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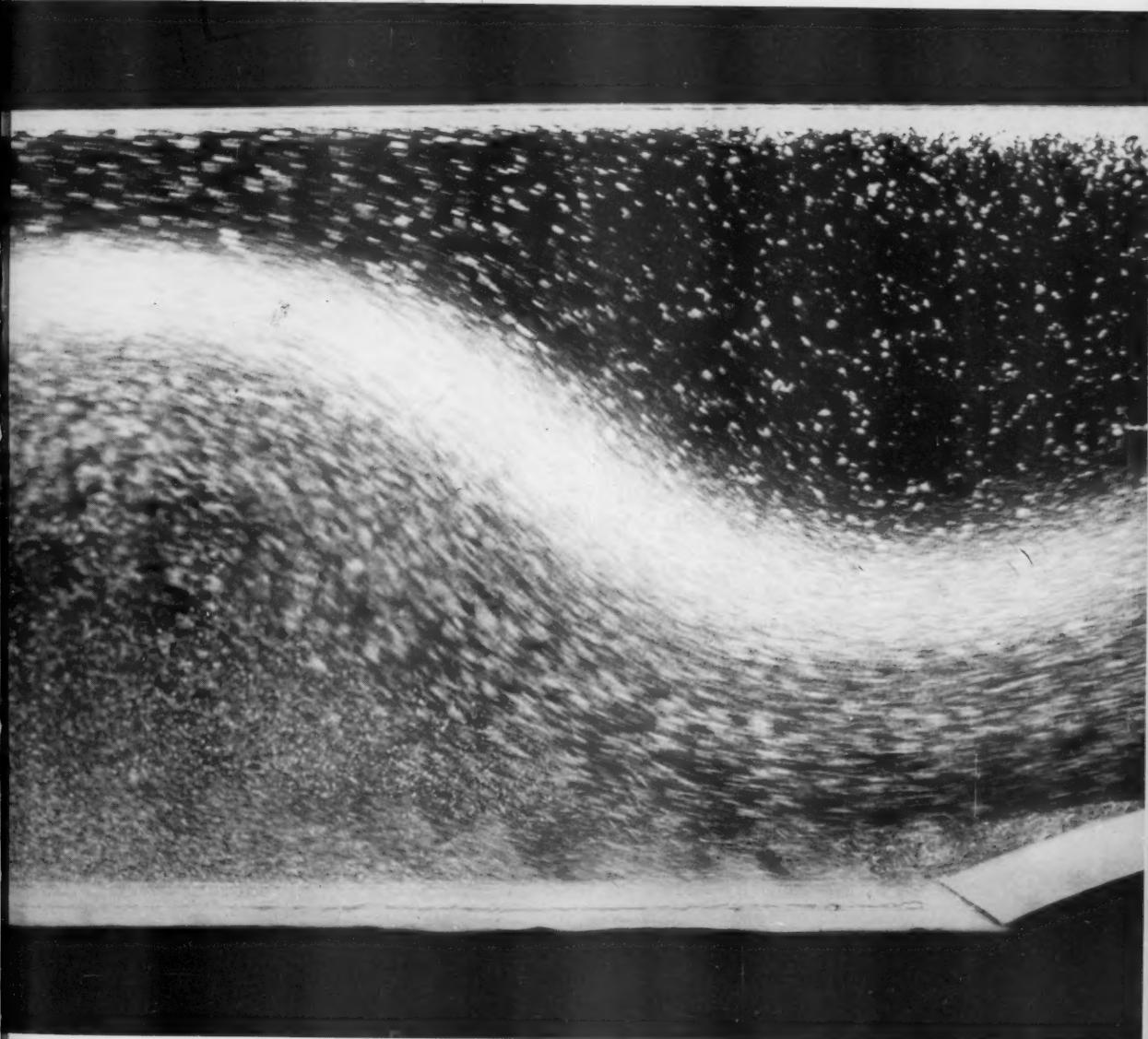
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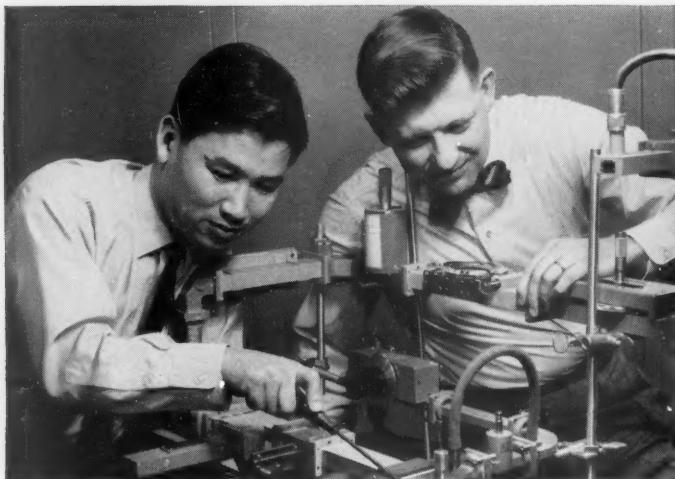
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29 April 1960

Vol. 131, No. 3409



THE IDEA THAT GREW FOR 100 YEARS



At Bell Laboratories, M. Uenohara (left) adjusts his reactance amplifier, assisted by A. E. Bakanowski, who helped develop first suitable diode. Extremely low "noise" is achieved when certain diodes are cooled in liquid nitrogen.

First practical diode for amplifier*, shown here held by tweezers, was jointly developed by A. E. Bakanowski and A. Uhliir.



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The new reactance amplifier has a busy future in the battle with "noise." At present, it is being developed for applications in tropospheric transmission and radar. But it has many other possible applications, as well. It can be used, for instance, in the reception of signals reflected from satellites. It is still another example of the continuing efforts to improve your Bell System communications.



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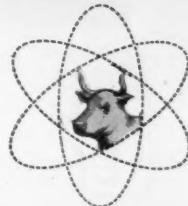
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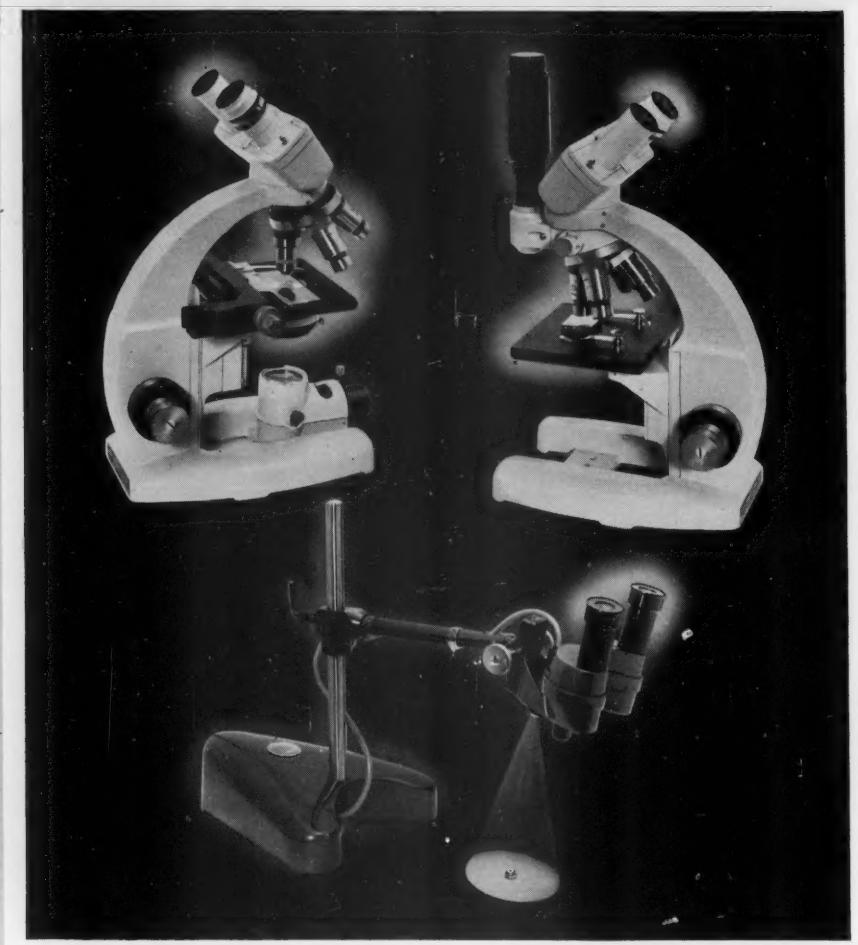
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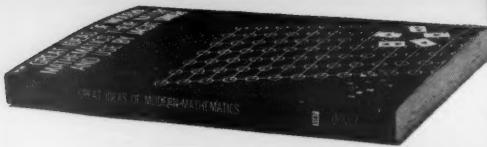
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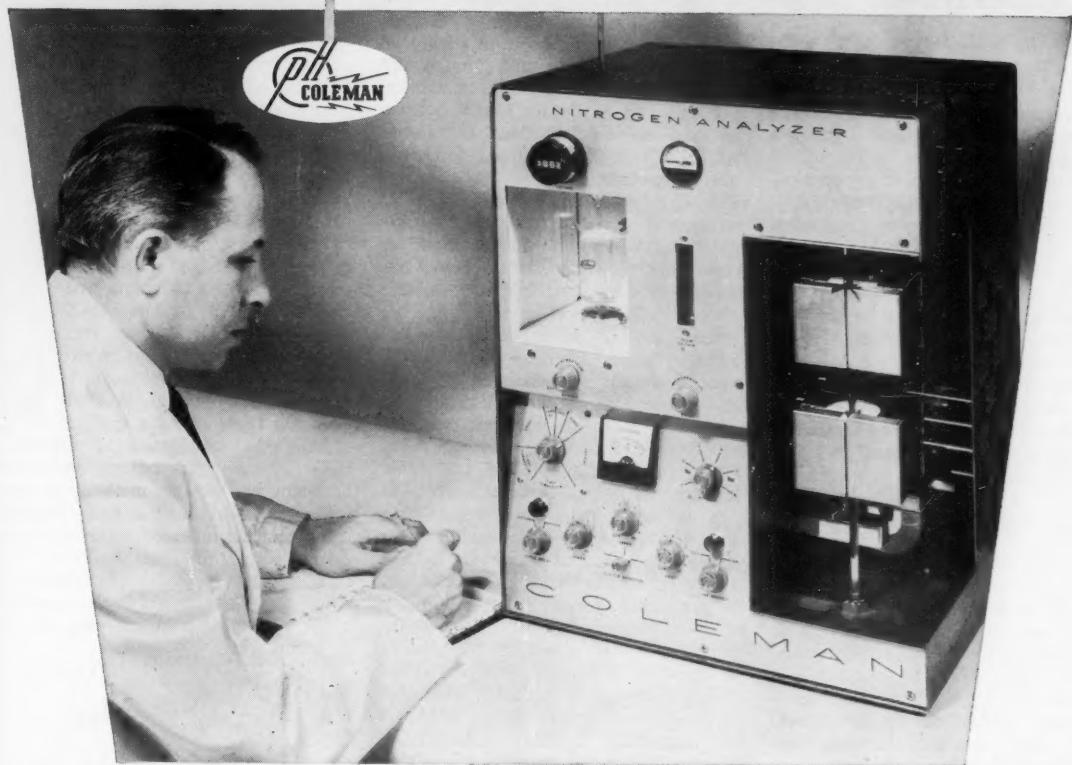
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OL. 131

Letters

Basic Research in France

I read in *Science* [128, 227(1958)] D. M. Gates' article on "Basic research in Europe." Now I am not a scientist; I can say that your analysis of research in France is quite right and is (alas!) a good account of reality.

One sentence must be, in my opinion, corrected; you say, "France is an agricultural country of peasant proprietorship" (p. 231).

This idea is deeply embedded in people's minds, even in France. It was true before World War I; it was not quite true at the end of World War II; it's not true today.

Although the proportion of farmers in the total population is higher than in Germany or in the United States, the evolution is rather swift, and in a few years the proportion will be nearly the same.

In my opinion, the low rank of French basic research in Europe is mainly explained by psychological reasons (individualism, fear of the modern mass world, and so on) rather than by industrial underdevelopment or the predominance of the peasant class.

My purpose here is to root out in a small way this legend that France produces only cognac, champagne, Montmartre girls, women's dresses, and licentious magazines.

ANDRÉ PAULY

Versailles, France

Food Preservation

I have been perplexed by the suggestion in the editorial of 12 February [*Science* 131, 383 (1960)] that "the Atomic Energy Commission should move to recover the Army fumble," which, it is alleged, occurred last October when the Army suspended its program on food sterilization by irradiation.

Science is an influential publication. I cannot be content to let this subject rest in the pages of *Science* on an editorial expression which, it seems to me, fails to embody adequate consideration of facts.

First, in order to avoid repetition in the matter of background, I wish to call attention to a statement I made in September 1955 on food sterilization methods [*Food Technol.* 9, 588 (1955)]. The essence of that statement was the conclusion "that heat will continue indefinitely to be the most effective and most suitable lethal agent for use in the sterilization of food." I still

stand on that statement, although now I would extend it.

The argument has been advanced in certain quarters that sterilization by irradiation has one clear-cut advantage over sterilization by heat, an advantage which justifies all the effort that would be required to "perfect" the irradiation process. This advantage is that, whereas foods sterilized by heat deteriorate while in storage, irradiated foods do not deteriorate.

In my statement, cited above, I wrote, "It appears unrealistic to expect that, among all the methods of preserving food, irradiation alone has the power to render the food incapable of quality deterioration in storage." I should like now to expand that statement by adding, "unless the composition of the food is changed radically and permanently by the process to such an extent that the unstable fractions are completely denatured or are converted into substances that are stable but are not natural to the food."

It is only logical to conclude that any treatment of food which so changes the food as to impart to it a degree of stability such as it possesses in no other form, must essentially effect an embalming of the food. By "embalming" is meant the imparting of chemical properties that are completely abnormal to the food, which, in all of its other known forms, needs to be protected against deteriorative changes by special means. The point is that if a substance possesses the stability that is claimed for irradiated food, the substance is no longer normal food. It might be added that, logically, one would suspect that this property of stability, if it exists, is directly associated with the undesirable organoleptic properties that are imparted to the food by irradiation and that if the undesirable properties are eliminated, the associated desirable property of stability would also be eliminated. Thus, of course, the only advantage of major importance that is still claimed for irradiation sterilization over heat sterilization would be eliminated.

Statistics on the matter are not available, but it is estimated that expenditures amounting to between \$2 and \$3 million have been made in developments aimed at bringing high-temperature, short-time sterilization into a commercially feasible and practicable form. Since 1927 I have been a participant in the work represented by a portion of those expenditures, and I can say that there has never been an effort put forth to advance heat sterilization which, even in a very small measure, resembled in concentration the effort, suspended last October, to develop irradiation sterilization. Never-

theless, the application of high-temperature, short-time sterilization is much further advanced than the application of ionizing radiation of food on any level.

In view of the greater promise offered by heat sterilization, I should like to offer a countersuggestion to that offered in the editorial. It is that the glamor of electronics be set aside, at least temporarily, in favor of an honest-to-goodness effort to bring advanced technology of heat sterilization to the point of practical application. Let one-third of the amount of \$14 million that was put into the unavailing effort to develop techniques of irradiation sterilization be allocated to a sincere investigation of the science and technology of high-temperature, short-time sterilization. With such an investigation, embodying a study of all potential types of heat application, including the techniques of dielectric, inductive, and infrared heating as well as direct and indirect methods of heating with fluid media and, perhaps, also the techniques of ultrasonics, the prospects are that, at the very least, such remarkable advances would be made that there would be no question about the wisdom of carrying the job to completion.

C. OLIN BALL

Rutgers University,
New Brunswick, New Jersey

Teaching and Research

F. J. Allen's comment [*Science* 131, 944 (1960)] that almost all scholars neglect their teaching for their research, and that we must come clean and admit it, cannot be allowed to stand. Surely the situation is this: no scholar who is not engaged in a struggle on the recalcitrant boundaries of his subject can generate the passion that first-rate teaching demands. Conversely, no scholar who is not both explaining the bases of his subject to students and concomitantly absorbing their fresh viewpoints can always keep his balance. The job of the scholar is not teaching or research. It is teaching-cum-research. In the best situations, it is a single activity—and that does not mean that one teaches only what one is momentarily engaged in investigating. Probably many of us neglect all or part of our jobs. But we do so, at least in part, because days are too short for us to perform this tremendous and joyful task in full accordance with our visions of it.

PAUL BOHANNAN

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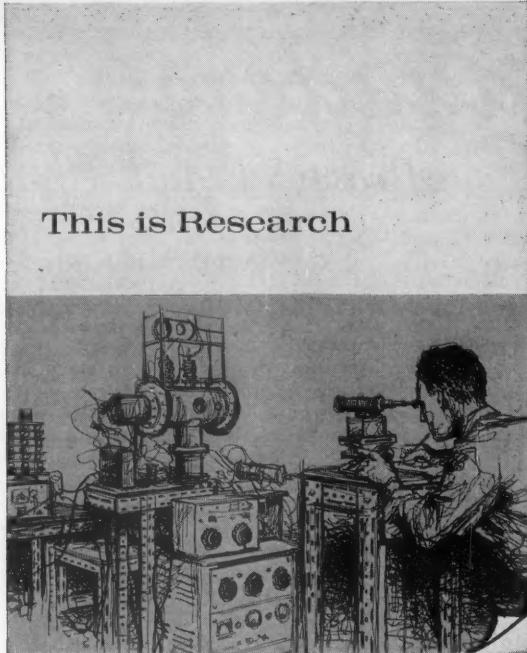
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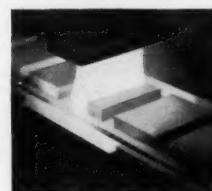
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Hazards of the '60's

Recent hearings before Representative John E. Fogarty's subcommittee of the House Committee on Appropriations brought out the difficulties inherent in getting congressional and public support for a broad-scale program in public health designed to meet the needs of the 1960's. Officials of the Public Health Service and other witnesses were in virtually complete agreement about what the hazards to health will be and about what will have to be done about them.

The hazards will increase as our expanding population concentrates more in urban areas. By 1970 the U.S. population will probably have increased by more than 30 million, the number of automobiles by some 20 million. Industry will undoubtedly increase at an even more rapid rate. The chemical industries, which pose special environmental problems, have grown exponentially in the last few decades and will probably continue to do so. From 1940 to 1959 the output of some representative chemicals increased as follows (in millions of pounds): plastics, 150 to 5000; detergents (which introduce special disposal problems), 15 to 1300; and insecticides and agricultural chemicals, 8 to 540. It is estimated that 400 to 500 new chemical products are put into use each year. In the production of these, new waste products are created. A continuing program of checking the toxicity of new products and by-products is a clear necessity.

The increasing use of x-rays in diagnosis and in industry, the rapidly expanding use of radioisotopes in industrial applications and research, and the increasing use of nuclear power will require control and monitoring. The disposal of radioactive wastes from nuclear facilities will demand more and more attention. All of these hazards, as well as others not mentioned, are designated environmental health problems by the Public Health Service.

What needs to be done? At present, according to Leroy E. Burney, surgeon general, the plans and activities developed by the Public Health Service for dealing with environmental hazards are carried out somewhat independently of each other, in response to special needs. "This has resulted in a loose grouping of related, but independently treated, problems, programs, and activities associated with the essential elements of our surroundings—the water we drink, the air we breathe, the food we eat, the shelter which protects us. . ." Burney proposed—and all witnesses concurred—that what is needed is an organization to consider the interrelated problems as a whole, an "Environmental Health Unit," which would integrate biomedical and engineering activities and bring about close relations between operational and research activities. Such a unit should carry on an expanded research program of its own but should also support research in universities.

Two steps are required: first, legislation to establish an Environmental Health Unit within the service, and second, adequate financing. The first step is easy, the second hard. And it is obvious why this is so. A specific disease can be dramatized: "Fight cancer with a check-up and a check!" But the environmental hazards are diffuse and difficult to personify in a way that will loosen congressional or individual purse strings. Fogarty remarked, "Environmental health doesn't seem to ring a bell with many people. . ." Burney agreed that the phrase lacked "drama or impact," but had no substitute to offer. Come in, Madison Avenue!—G.DuS.

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CURRENT PROBLEMS IN RESEARCH

The Atmosphere in Motion

Research in geophysical fluid mechanics shows how density variation and rotation affect air motions.

Robert R. Long

The atmosphere is a fluid; it is a gas, pressed against the spinning earth by the force of gravity, heated by the sun and, as its moisture in gaseous form is turned into cloud and mist, by the latent heat of condensation. If the heating process is looked upon as known, study of its motion is a branch of fluid mechanics—physics in 1860, engineering science in 1960.

Until recently, research in meteorology was dominated, even oppressed, by the needs of weather forecasting. An understanding of the simplest effects of rotation and of density and temperature variations must surely precede an understanding of atmospheric phenomena containing these and a host of other features, but the relationship of such basic research to forecasting was considered too remote. Meteorologists hoped, rather, to find their prediction tools by studying the drift and development of the isobars on their weather maps, or by a variety of careful or casual statistical approaches.

But, it can be argued that all progress in forecasting since 1920—and it has been very modest—has come from looking more closely and more frequently at our atmosphere—that is, from improved observations rather than from research efforts. If the meteorologist will not accept this proposition, at least he will acknowledge that the returns from the vast amount of empirical synoptic and statistical research of the past 40 years have been astonishingly small. In other words, we are on a plateau and have been on it for many

years. The situation is not changed, incidentally, by the advent in the past decade of numerical weather forecasting; the Weather Bureau forecaster now adds the output of the routine numerical integration to his other, older forecasting tools, but the improvement is too slight for much optimism.

Clearly, future progress demands fundamental research. Some basic work in what might be called geophysical fluid mechanics is now going on. I mean by this term, motion of fluids whose elements have spin, for example those disturbed in some way from a basic state of solid rotation, and motion of fluids in which density variations are present and important. A reservoir of knowledge is lacking in this field. True, fluid mechanics is an old subject, but classical work of the 19th century (1) was limited precisely to fluids in which rotation and density differences are absent. More recent work (2) has stemmed from the needs of aerodynamics, to a large extent, and these differ fundamentally from the needs of meteorology, oceanography, or astrophysics.

Geophysical Fluid Mechanics

Ruthlessly simplified, two simple items form the backbone of geophysical fluid mechanics. The first has to do with the motion of a particle as it appears to an observer on a rotating platform (3). To illustrate, suppose a particle is projected straight out from the

center of rotation. If it is under no forces, the resting observer sees its path as a straight line, the dashed line in Fig. 1. He sees the platform turn underneath the moving particle, but this does not influence his measurement of the particle's path. The observer on the platform sees the particle curve to the right, however; to preserve the form of Newton's second law, "force equals mass times acceleration," he imagines that a force is acting, pushing the particle to the right. This is the Coriolis force; its presence, arising from the earth's rotation, is responsible for the fascination and the difficulty of much of geophysical fluid mechanics.

We mentioned a second feature; this is the typical variation of density of geophysical fluid systems. An example is ocean water, cold and therefore heavy near the ocean floor, warm and therefore light near the surface. Such density stratification brings with it a great resistance to vertical motion (4). If a cold, heavy particle of water is forced to rise by some disturbance or other, it finds itself surrounded by warm, light material, and it tends to sink again. The particle will usually drop below its natural level on the way down; if it does so, it will then find itself embedded in colder and denser fluid, and it will be forced back up. Not only does this work in favor of horizontal motion, but the repeated overshooting, an answer to a disturbance, is a wave phenomenon of separate interest and importance.

Air, the material of the atmosphere, is compressible; as it rises or falls, its density decreases or increases as the surrounding air presses on it less heavily or more heavily. But the principle is the same. If forced to rise, a parcel of air will normally find its surroundings lighter, and it will begin to sink; if forced to drop below its level of origin, it will normally find its surroundings denser, and it will be pushed back up. I will refer to these restoring forces as buoyancy forces.

The science of fluid mechanics is unusual in one respect; we have precise

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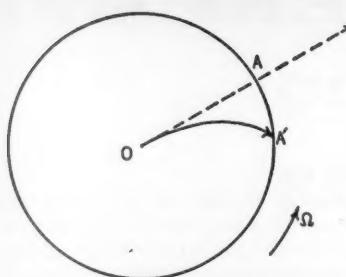


Fig. 1. Paths of a particle shot out from the axis of a rotating disk. The fixed observer sees the path as the straight, dashed line. However, the point of the disk A' has moved to the fixed point A while the particle is traveling from the center to the edge of the disk; the moving observer sees the path as the curve OA' .

mathematical expressions for the laws governing the motion of fluids, and these laws have passed with flying colors nearly all tests of their accuracy (5). Even in the sister science, solid mechanics, the equations of motion are not known for any real material. In a sense the whole of fluid mechanics is reduced to finding solutions of a mathematically determinate set of differential equations, and, in fact, applied mathematicians have been especially attracted to this field. But two things spoil this neat pic-

ture: first, only a few solutions of the equations have ever been found, despite a century of searching (6); second, the natural state of fluid motions, especially geophysical motions, is so disordered or turbulent (7) that it is unreasonable to ask for a knowledge of the motion in all its detail. The second point suggests that what we really want to know is some kind of average values of speed, temperature, and so on. But as far as we have developed them to date, the equations governing these average motions do not determine the problem in a mathematical sense.

We find ourselves, then, in a situation not differing much from that of other scientists, and we respond to our limitations in similar ways: we resort to both simplification and experiment. Experiment, however, despite its prominent role in almost all forms of scientific inquiry, has not been used much in geophysics, because it was thought impossible to reduce such vast systems to the scale of the laboratory (8). Recent work has shown that this estimate is overly pessimistic; in any event, it is unnecessary to claim that a little whirlpool generated in a rotating vessel of water in the laboratory is identical to a tornado in order to justify a scientist's giving it the second look. These assertions are justified below.

Stratified Flows

Experimentally, we may study the interplay of density variations and gravity in many ways. I limit myself to one, described in some detail. The geophysical motivation becomes obvious as the picture unfolds.

The experimental set-up is a glass-walled channel, 20 feet long, 2 feet deep, and 6 inches wide (9). We fill it with salt and water, so mixed that the saltiness, and therefore the density, decreases uniformly from bottom to top. Now we have a stratified fluid like the water in the ocean or the air in the atmosphere, although, in both of these fluids, density variations stem from temperature rather than salinity. Suppose now we put a small obstacle of some "easy" shape in the channel and pull it along the bottom at a fixed speed. The disturbance it causes would fascinate any person curious about the ways of nature. The geophysicist too is intrigued, but he prefers to look at the motion as he (actually, as a camera) sees it, moving along with the obstacle on a track parallel to the channel walls. In this way, he sees a current of fluid, with density decrease in the vertical, moving at a steady speed over a "fixed" barrier. Now we have something meteorological, because this reproduces the essentials of

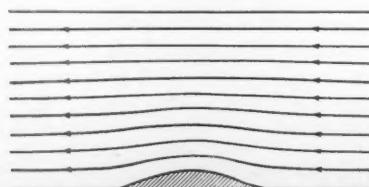


Fig. 2. Flow of pure water over an obstacle at the bottom of a channel. The flow is shown by putting an aluminum powder in the water and taking a photograph with a 1-second time exposure. Each streak is the path of a particle during this time period. The direction of the streak gives the direction of the motion; the length of the streak gives the speed.

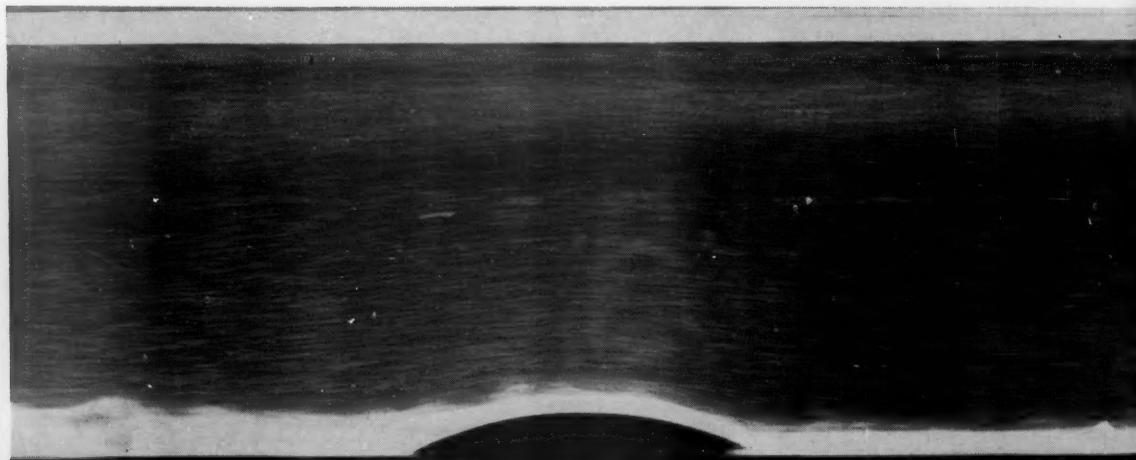
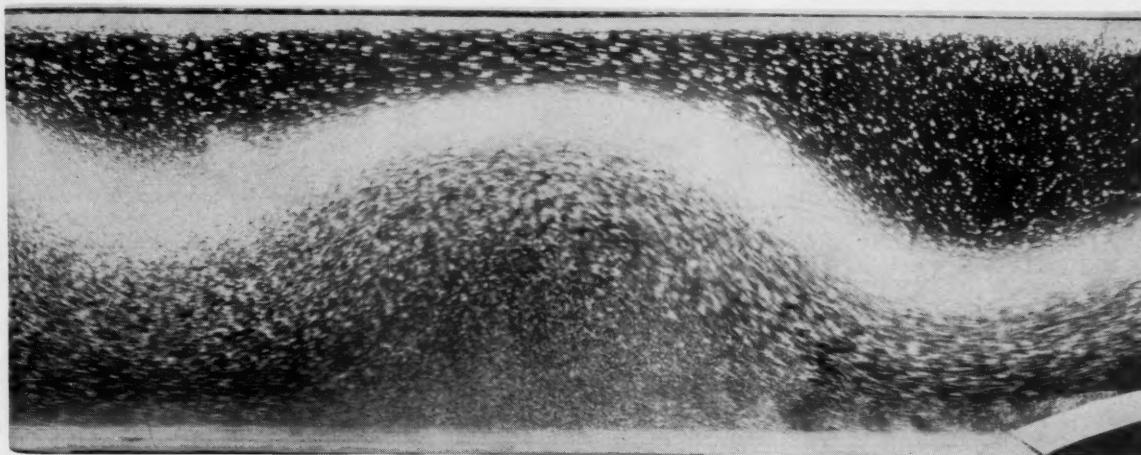
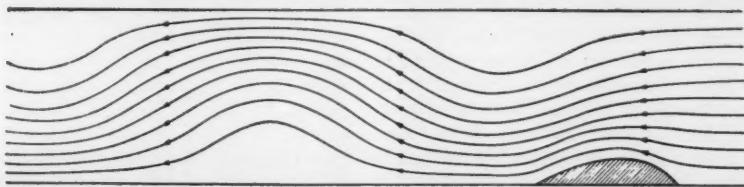


Fig. 3. Flow of a salt-water mixture over an obstacle. The undisturbed density variation in this experiment is a linear decrease with height. The line drawing is the plot of the flow obtained from a theoretical solution of the equations of motion.



air motion over mountain barriers on the ground.

As we have hinted, the density variation has a startling effect. If the fluid is pure water, the flow is like that in Fig. 2. But density variation produces an infinite variety of motion, depending on the speed of the basic current. If the current is very fast, the density variation has little or no influence, and the flow resembles Fig. 2 again. If the current is moderately slow, we have simple wave patterns, as in Fig. 3. The waves are explainable in a general way by the action of the buoyancy force that we have already discussed, but specific patterns can be very involved.

It was evident that the influence of internal friction on the flow patterns could be neglected, at least as a first approximation. With this and other minor simplifications, it was possible to find solutions for the flow. An example is the line drawing in Fig. 3, one of the myriad of flow patterns revealed by the theory. This particular pattern was chosen because the parameters which yield this theoretical flow pattern are the same as the parameters in this particular experiment. The agreement is excellent, as it is in all the comparisons of theory and experiment in this investigation.

But if theory and experiment are the

same for a wide range of simple conditions, we are encouraged to rely solely on observations of the experiment if conditions (complicated obstacle forms) are so complicated that a theoretical solution is beyond our mathematical abilities. We might get even bolder than this and attempt to produce in our little channel a model of air flow over detailed terrain, or a model of the motion of tidal currents over a rough ocean bottom.

A particular case was chosen—air flow over the Sierra Nevadas in eastern California (10). Here we have a long ridge of mountains 12,000 to 14,000 feet high, running roughly north-south. The ground rises gradually from the Sacramento Valley to the west. East of the ridge it drops about 10,000 feet in a few miles, down to the Owens Valley. This valley is limited to the east by another very high ridge called the White Mountains. The Owens Valley is the scene of spectacular cloud forms and air motions. During days of strong activity a huge rotating cloud sits over the valley; motions in and around it are incredibly violent. Vertical velocities are so great that a light plane even in a full power dive may still be swept upward. World's altitude records for gliders are set here, and pilots could go still higher if cabins were pressurized.

The greatest activity comes when strong winds from the Pacific blow over the ridge. The air cascades down the lee and then rises in a series of standing waves. The motion has been studied carefully (11) in the Owens Valley near Bishop, California; the flow on a day of strong activity is pictured in Fig. 4. Compare this with the flow in the experimental model of Fig. 5. In the model an obstacle was made with a shape similar to the terrain in this area, and a certain number, called F_1 , was chosen to be the same in the model as during this day in the region of the Sierras. (Equality of this number, which involves the basic speed of the current, gravity, and a measure of the density variation, is a necessary condition for similarity of the flow patterns.)

The agreement between the observed and the model flows is important. It is the first really definite indication, I think, that we can use in our geophysical studies this very powerful tool—modeling—which has proved so useful in engineering (in the design of aircraft, surface vessels, dams, and so on). In our case, all possible patterns of flow over the ridge could be found by a methodical variation of conditions of the experiment. A comparison of this catalog of findings with the forecast of "upstream" wind and temperature condi-

tions on a given day would lead directly to a forecast of weather conditions in the Owens Valley. The same approach could be used for any locality in the world whose weather is substantially influenced by local terrain. This discussion of stratified flow shows how a piece of basic research can lead naturally, almost inevitably, to the solution of a practical problem.

The Tornado Vortex

A second example of the interplay of theory and experiment is in a recent study of vortices. The atmosphere has vortices of many scales, from the extra-tropical cyclone covering half a continent to the tiny dust whirls that form over a patch of hot ground on a summer day. But the investigations I report

on here bear most directly on the tornado (12). The tornado is perhaps the most spectacular and dreadful display of nature. Its winds must reach speeds of several hundreds of miles per hour, but they are so dangerous and destructive that we cannot enter the tornado to measure them. Nor is high wind the only danger; the pressure at the center drops so low so quickly that build-

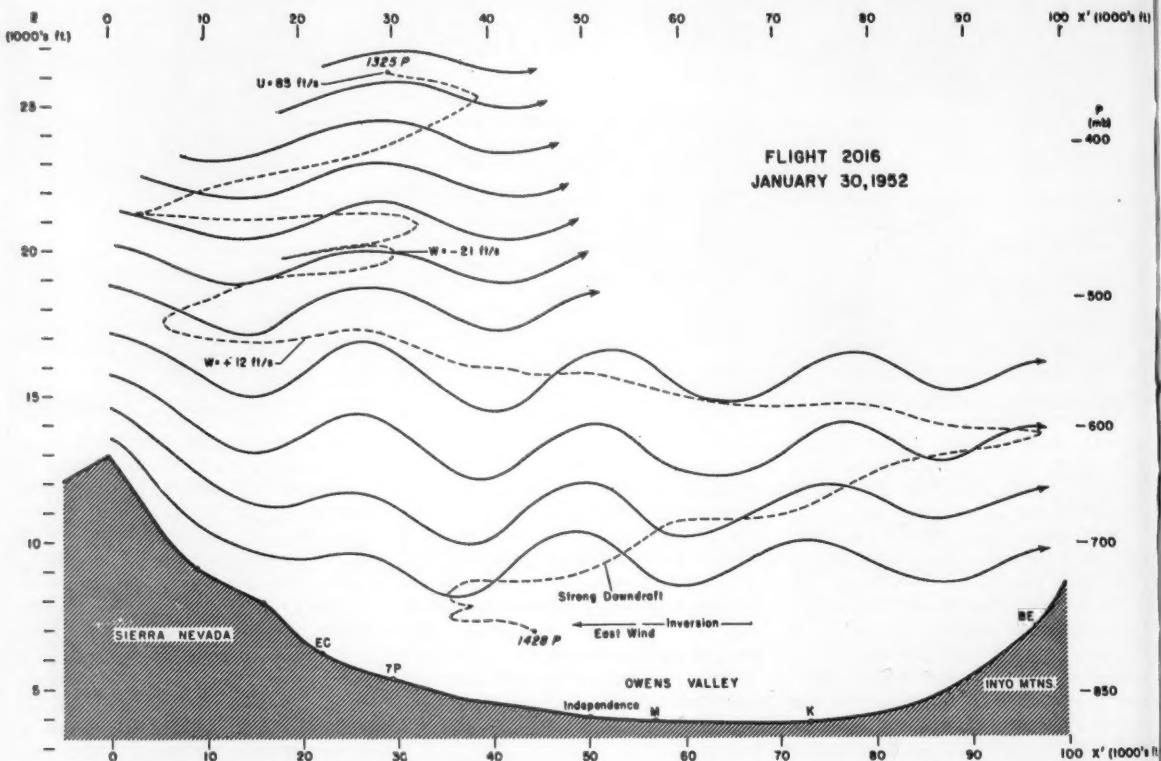


Fig. 4. Streamlines of air flow over the Sierra Nevada mountains on the afternoon of 30 January 1952, as determined by members of the department of meteorology, University of California, Los Angeles. The vertical dimensions of the atmosphere are greatly exaggerated in this drawing.

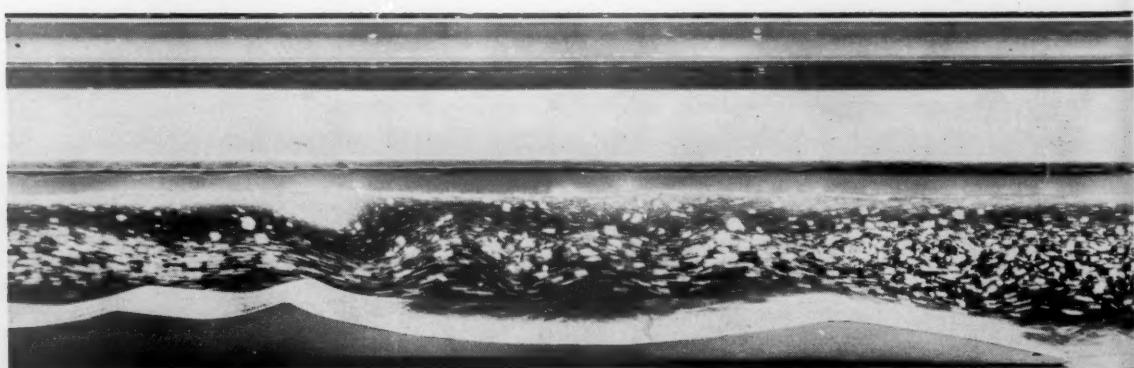


Fig. 5. Experimental model of air flow over the Sierra Nevadas. "Upstream" conditions were chosen to be as close as possible to those of Fig. 4. Layers of fluid on top are immiscible liquids lighter than salt water. They reproduce the very stable stratosphere.

ings explode outward as the vortex passes by. The practical importance of a study of tornadoes is obvious. They are now so unpredictable that the Weather Bureau can rarely provide even minimum warning.

The vortex study was begun with quite another end in mind. The original intention was to extend to a fluid in rotation a classical hydrodynamic study of the way in which fluid in a container moves toward a small hole in the wall of the container. This is a hydrodynamic sink, and although it may seem at first glance to be a trivial and uninteresting form of motion, it turns out to be of the greatest possible importance in fluid mechanics. This granted, a valuable study in rotating fluids might well be how water in a cylinder mounted on a rotating turntable flows toward a hole in the bottom of the vessel. Although few problems in rotating fluids can be solved theoretically, this one can if the withdrawal rate is above a minimum value and if friction is unimportant. If we pull out fluid fast enough, the movement of the water toward the hole is largely unaffected by the rotation. Lower the rate, and the sink draws more and more strongly from along the axis, until finally the water approaches the hole in a jet just at the axis. Here we have already the beginnings of a vortex because the fluid in the jet near the axis originates some distance away, and as it moves in, conservation of angular momentum means a considerable increase in spin.

The mathematical solutions resembled very closely the corresponding experimental flow. Although the theory was limited by the requirement that the withdrawal rate be above a certain value, no such limitation existed for the experiment. When we withdrew fluid at a rate lower than the theoretical minimum, the vortex at the axis became more and more concentrated and more and more intense. Eventually we got a remarkably strong vortex in which the spin was hundreds or thousands of times greater than before withdrawal began. Some idea of the motion can be gained from Fig. 6.

For the first time in our discussion we must take into account internal friction in the fluid. Friction is fundamental in fluids whenever velocity variations get large enough. In this vortex, for example, we see (Fig. 6) that speeds are enormous in a small core and drop off precipitately as we go out from the center. It was easy to make a quantitative estimate that friction is essential

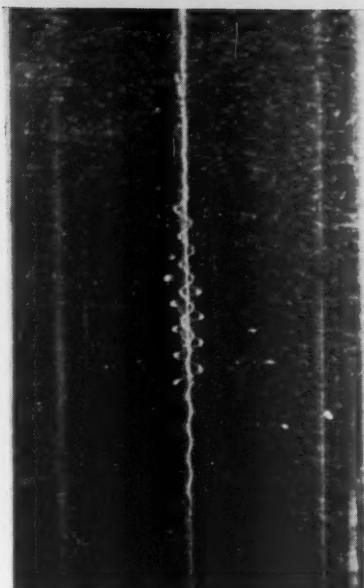


Fig. 6. Vortex at the axis of a rotating vessel of water. This 1/5-second time exposure shows that particles make several revolutions as they progress along the axis.

in the intense vortex, and this set off an investigation that led to a new theory of vortices.

In a recent paper this theory was compared to the tornado phenomenon (13), with some remarkable conclusions

about its form, its velocity field, and its causes. The most striking discovery is that the air must be rushing violently upward in the center of a tornado at speeds of several hundred miles an hour. Superimposed, of course, is the swirling velocity, which is also 200 or 300 miles an hour or so. The strong upward velocities were to some extent unexpected, although one observer once saw a tornado outrun the cloud in which it was embedded; while he watched it, the visible portion shot up to 35,000 feet in 1 minute (12). This implies a vertical velocity of about 400 miles per hour, as the theoretical model suggests.

The theory yields a picture of the funnel cloud of the tornado if we make the reasonable assumption that it is a surface of constant pressure in the lower layers. A particular case is shown in Fig. 7. If we take the funnel to be the 900-millibar surface, it has a diameter of several hundred feet as commonly observed, and its shape is very similar to the typical tornado funnel.

The theory permits an interesting speculation about the necessary conditions for the occurrence of a tornado. To get a theoretical vortex of the right intensity and size we must insert in the theory values of a number of atmospheric quantities—for example, density, ground pressure, and viscosity coefficient of the turbulent air. All of the

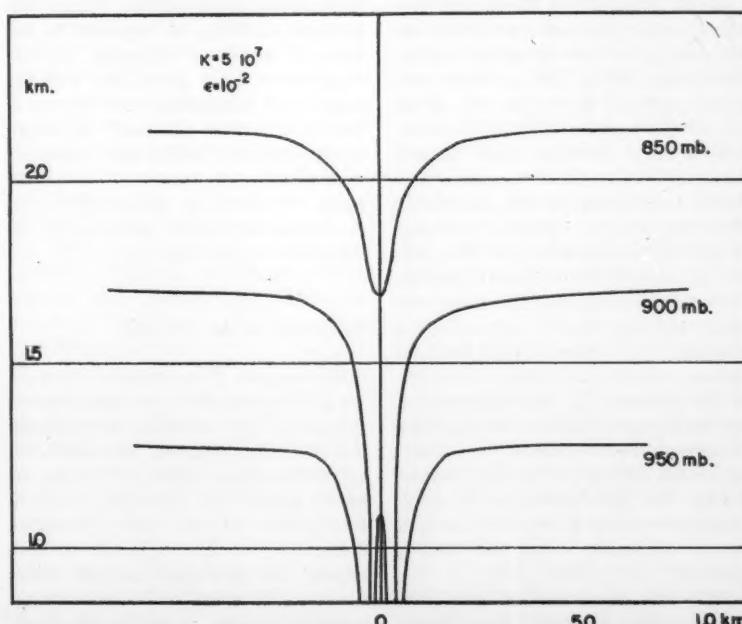


Fig. 7. Constant-pressure surfaces in a typical theoretical tornado. It is reasonable to take the 900-millibar surface as the outer surface of the tornado funnel.

parameters have values typical of, say, any warm, moist air mass found in spring and summer in the central United States, with a single exception, a quantity K called *circulation*, which is a measure of the general rotation of the air in which the tornado is imbedded. This quantity has been estimated with great accuracy for at least one tornado, and I think we know very closely its value in the typical case. It corresponds, however, to so great a rotation that it is obviously a very rare occurrence. This may explain the infre-

quency of tornadoes. It is possible that we could learn to predict this (parent) small-scale cyclone, and this in turn could lead to better forecasting of tornadoes.

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The Competitive Exclusion Principle

An idea that took a century to be born has implications in ecology, economics, and genetics.

Garrett Hardin

On 21 March 1944 the British Ecological Society devoted a symposium to the ecology of closely allied species. There were about 60 members and guests present. In the words of an anonymous reporter (1), "a lively discussion . . . centred about Gause's contention (1934) that two species with similar ecology cannot live together in the same place. . . . A distinct cleavage of opinion revealed itself on the question of the validity of Gause's concept. Of the main speakers, Mr. Lack, Mr. Elton and Dr. Verley supported the postulate. . . . Capt. Diver made a vigorous attack on Gause's concept, on the grounds that the mathematical and experimental approaches had been dangerously oversimplified. . . . Pointing out the difficulty of defining 'similar ecology' he gave examples of many congruent species of both plants and animals apparently living and feeding together."

Thus was born what has since been called "Gause's principle." I say "born" rather than "conceived" in order to draw an analogy with the process of

mammalian reproduction, where the moment of birth, of exposure to the external world, of becoming a fully legal entity, takes place long after the moment of conception. With respect to the principle here discussed, the length of the gestation period is a matter of controversy: 10 years, 12 years, 18 years, 40 years, or about 100 years, depending on whom one takes to be the father of the child.

Statement of the Principle

For reasons given below, I here refer to the principle by a name already introduced (2)—namely, the "competitive exclusion principle," or more briefly, the "exclusion principle." It may be briefly stated thus: *Complete competitors cannot coexist*. Many published discussions of the principle revolve around the ambiguity of the words used in stating it. The statement given above has been very carefully constructed: every one of the four words is ambiguous. This formulation has

been chosen not out of perversity but because of a belief that it is best to use that wording which is least likely to hide the fact that we still do not comprehend the exact limits of the principle. For the present, I think the "threat of clarity" (3) is a serious one that is best minimized by using a formulation that is *admittedly* unclear; thus can we keep in the forefront of our minds the unfinished work before us. The wording given has, I think, another point of superiority in that it seems brutal and dogmatic. By emphasizing the very aspects that might result in our denial of them were they less plain we can keep the principle explicitly present in our minds until we see if its implications are, or are not, as unpleasant as our subconscious might suppose. The meaning of these somewhat cryptic remarks should become clear further on in the discussion.

What does the exclusion principle mean? Roughly this: that (i) if two noninterbreeding populations "do the same thing"—that is, occupy precisely the same ecological niche in Elton's sense (4)—and (ii) if they are "sympatric"—that is, if they occupy the same geographic territory—and (iii) if population A multiplies even the least bit faster than population B , then ultimately A will completely displace B , which will become extinct. This is the "weak form" of the principle. Always in practice a stronger form is used, based on the removal of the hypothetical character of condition (iii). We do this because we adhere to what may be called the axiom of inequality, which states that no two things or processes,

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in a real world, are precisely equal. This basic idea is probably as old as philosophy itself but is usually ignored, for good reasons. With respect to the things of the world the axiom often leads to trivial conclusions. One postage stamp is as good as another. But with respect to competing processes (for example, the multiplication rates of competing species) the axiom is never trivial, as has been repeatedly shown (5-7). No difference in rates of multiplication can be so slight as to negate the exclusion principle.

Demonstrations of the formal truth of the principle have been given in terms of the calculus (5, 7) and set theory (8). Those to whom the mathematics does not appeal may prefer the following intuitive verbal argument (2, pp. 84-85), which is based on an economic analogy that is very strange economics but quite normal biology.

"Let us imagine a very odd savings bank which has only two depositors. For some obscure reason the bank pays one of the depositors 2 percent compound interest, while paying the other 2.01 percent. Let us suppose further (and here the analogy is really strained) that whenever the sum of the combined funds of the two depositors reaches two million dollars, the bank arbitrarily appropriates one million dollars of it, taking from each depositor in proportion to his holdings at that time. Then both accounts are allowed to grow until their sum again equals two million dollars, at which time the appropriation process is repeated. If this procedure is continued indefinitely, what will happen to the wealth of these two depositors? A little intuition shows us (and mathematics verifies) that the man who receives the greater rate of interest will, in time, have all the money, and the other man none (we assume a penny cannot be subdivided). No matter how small the difference between the two interest rates (so long as there is a difference) such will be the outcome.

"Translated into evolutionary terms, this is what competition in nature amounts to. The fluctuating limit of one million to two million represents the finite available wealth (food, shelter, etc.) of any natural environment, and the difference in interest rates represents the difference between the competing species in their efficiency in producing offspring. No matter how small this difference may be, one species will eventually replace the other. In the scale of geological time, even a

small competitive difference will result in a rapid extermination of the less successful species. Competitive differences that are so small as to be unmeasurable by direct means will, by virtue of the compound-interest effect, ultimately result in the extinction of one competing species by another."

The Question of Evidence

So much for the theory. Is it true? This sounds like a straightforward question, but it hides subtleties that have, unfortunately, escaped a good many of the ecologists who have done their bit to make the exclusion principle a matter of dispute. There are many who have supposed that the principle is one that can be proved or disproved by empirical facts, among them (9, 10) Gause himself. Nothing could be farther from the truth. The "truth" of the principle is and can be established only by theory, not being subject to proof or disproof by facts, as ordinarily understood. Perhaps this statement shocks you. Let me explain.

Suppose you believe the principle is true and set out to prove it empirically. First you find two noninterbreeding species that seem to have the same ecological characteristics. You bring them together in the same geographic location and await developments. What happens? Either one species extinguishes the other, or they coexist. If the former, you say, "The principle is proved." But if the species continue to coexist indefinitely, do you conclude the principle is false? Not at all. You decide there must have been some subtle difference in the ecology of the species that escaped you at first, so you look at the species again to try to see how they differ ecologically, all the while retaining your belief in the exclusion principle. As Gilbert, Reynoldson, and Hobart (10) dryly remarked, "There is . . . a danger of a circular process here . . ."

Indeed there is. Yet the procedure can be justified, both empirically and theoretically. First, empirically. On this point our argument is essentially an acknowledgement of ignorance. When we think of mixing two similar species that have previously lived apart, we realize that it is hardly possible to know enough about species to be able to say, in advance, which one will exclude the other in free competition. Or, as Darwin, at the close of chapter 4 of his *Origin of Species* (11) put it:

"It is good thus to try in imagination to give any one species an advantage over another. Probably in no single instance should we know what to do. This ought to convince us of our ignorance on the mutual relations of all organic beings: a conviction as necessary, as it is difficult to acquire."

How profound our ignorance of competitive situations is has been made painfully clear by the extended experiments of Thomas Park and his collaborators (12). For more than a decade Park has put two species of flour beetles (*Tribolium confusum* and *T. castaneum*) in closed universes under various conditions. In every experiment the competitive exclusion principle is obeyed—one of the species is completely eliminated, but it is not always the same one. With certain fixed values for the environmental parameters the experimenters have been unable to control conditions carefully enough to obtain an invariable result. Just how one is to interpret this is by no means clear, but in any case Park's extensive body of data makes patent our immense ignorance of the relations of organisms to each other and to the environment, even under the most carefully controlled conditions.

The theoretical defense for adhering come-hell-or-high-water to the competitive exclusion principle is best shown by apparently changing the subject. Consider Newton's first law: "Every body persists in a state of rest or of uniform motion in a straight line unless compelled by external force to change that state." How would one verify this law, by itself? An observer might (in principle) test Newton's first law by taking up a station out in space somewhere and then looking at all the bodies around him. Would any of the bodies be in a state of rest except (by definition) himself? Probably not. More important, would any of the bodies in motion be moving in a straight line? *Not one*. (We assume that the observer makes errorless measurements.) For the law says, ". . . in a straight line unless compelled by external force to change . . .," and in a world in which another law says that "every body attracts every other body with a force that is inversely proportional to the square of the distance between them . . .," the phrase in the first law that begins with the words *unless compelled* clearly indicates the hypothetical character of the law. So long as there are no sanctuaries from gravitation in space, every body is al-

ways "compelled." Our observer would claim that any body at rest or moving in a straight line verified the law; he would likewise claim that bodies moving in not-straight lines verified the law, too. In other words, any attempt to test Newton's first law by itself would lead to a circular argument of the sort encountered earlier in considering the exclusion principle.

The point is this: We do not test isolated laws, one by one. What we test is a whole conceptual model (13). From the model we make predictions; these we test against empirical data. When we find that a prediction is not verifiable we then set about modifying the model. There is no procedural rule to tell us which element of the model is best abandoned or changed. (The scientific response to the results of the Michelson-Morley experiment was not in any sense determined.) Esthetics plays a part in such decisions.

The competitive exclusion principle is one element in a system of ecological thought. We cannot test it directly, by itself. What the whole ecological system is, we do not yet know. One immediate task is to discover the system, to find its elements, to work out their interactions, and to make the system as explicit as possible. (Complete explicitness can never be achieved.) The works of Lotka (14), Nicholson (15, 16), and MacArthur (17) are encouraging starts toward the elaboration of such a theoretical system.

The Issue of Eponymy

That the competitive exclusion principle is often called "Gause's principle" is one of the more curious cases of eponymy in science (like calling human oviducts "Fallopian tubes," after a man who was not the first to see them and who misconstrued their significance). The practice was apparently originated by the English ecologists, among whom David Lack has been most influential. Lack made a careful study of *Geospiza* and other genera of finches in the Galápagos Islands, combining observational studies on location with museum work at the California Academy of Sciences. How his ideas of ecological principles matured during the process is evident from a passage in his little classic, *Darwin's Finches* (18).

"Snodgrass concluded that the beak differences between the species of *Geo-*

spiza are not of adaptive significance in regard to food. The larger species tend to eat rather larger seeds, but this he considered to be an incidental result of the difference in the size of their beaks. This conclusion was accepted by Gifford (1919), Gulick (1932), Swarth (1934) and formerly by myself (Lack, 1945). Moreover, the discovery . . . that the beak differences serve as recognition marks, provided quite a different reason for their existence, and thus strengthened the view that any associated differences in diet are purely incidental and of no particular importance.

"My views have now completely changed, through appreciating the force of Gause's contention that two species with similar ecology cannot live in the same region (Gause, 1934). This is a simple consequence of natural selection. If two species of birds occur together in the same habitat in the same region, eat the same types of food and have the same other ecological requirements, then they should compete with each other, and since the chance of their being equally well adapted is negligible, one of them should eliminate the other completely. Nevertheless, three species of ground-finches live together in the same habitat on the same Galapagos islands, and this also applies to two species of insectivorous tree-finches. There must be some factor which prevents these species from effectively competing."

Implicit in this passage is a bit of warm and interesting autobiography. It is touching to see how intellectual gratitude led Lack to name the exclusion principle after Gause, calling it, in successive publications, "Gause's contention," "Gause's hypothesis," and "Gause's principle." But the eponymy is scarcely justified. As Gilbert, Reynoldson, and Hobart point out (10, p. 312): "Gause . . . draws no general conclusions from his experiments, and moreover, makes no statement which resembles any wording of the hypothesis which has arisen bearing his name." Moreover, in the very publication in which he discussed the principle, Gause acknowledged the priority of Lotka in 1932 (5) and Volterra in 1926 (6). Gause gave full credit to these men, viewing his own work merely as an empirical testing of their theory—a quite erroneous view, as we have seen. How curious it is that the principle should be named after a man who did not state it clearly, who mis-

apprehended its relation to theory, and who acknowledged the priority of others!

Recently Udvardy (19), in an admirably compact note, has pointed out that Joseph Grinnell, in a number of publications, expressed the exclusion principle with considerable clarity. In the earliest passage that Udvardy found, Grinnell, in 1904 (20), said: "Every animal tends to increase at a geometric ratio, and is checked only by limit of food supply. It is only by adaptations to different sorts of food, or modes of food getting, that more than one species can occupy the same locality. Two species of approximately the same food habits are not likely to remain long enough evenly balanced in numbers in the same region. One will crowd out the other."

Udvardy quotes from several subsequent publications of Grinnell, from all of which it is quite clear that this well-known naturalist had a much better grasp of the exclusion principle than did Gause. Is this fact, however, a sufficiently good reason for now speaking (as Udvardy recommends) of "Grinnell's axiom?" On the basis of present evidence there seems to be justice in the proposal, but we must remember that the principle has already been referred to, in various publications, as "Gause's principle," the "Volterra-Gause principle," and the "Lotka-Volterra principle." What assurance have we that some diligent scholar will not tomorrow unearth a predecessor of Grinnell? And if this happens, should we then replace Grinnell's name with another's? Or should we, in a fine show of fairness, use all the names? According to this system, the principle would, at present, be called the Grinnell-Volterra-Lotka-Gause-Lack principle—and, even so, injustice would be done to A. J. Nicholson, who, in his wonderful gold mine of unexploited aphorisms (15), wrote: "For the steady state [in the coexistence of two or more species] to exist, each species must possess some advantage over all other species with respect to some one, or group, of the control factors to which it is subject." This is surely a corollary of the exclusion principle.)

In sum, I think we may say that arguments for pinning an eponym on this idea are unsound. But it does need a name of some sort; its lack of one has been one of the reasons (though not the only one) why this basic principle has trickled out of the scientific con-

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sciousness after each mention during the last half century. Like Allee *et al.* (21) we should wish "to avoid further implementation of the facetious definition of ecology as being that phase of biology primarily abandoned to terminology." But, on the other side, it has been pointed out (22): "Not many recorded facts are lost; the bibliographic apparatus of science is fairly equal to the problem of recording melting points, indices of refraction, etc., in such a way that they can be recalled when needed. Ideas, more subtle and more diffusely expressed present a bibliographic problem to which there is no present solution." To solve the bibliographic problem some sort of handle is needed for the idea here discussed; the name "the competitive exclusion principle" is correctly descriptive and will not be made obsolete by future library research.

The Exclusion Principle and Darwin

In our search for early statements of the principle we must not pass by the writings of Charles Darwin, who had so keen an appreciation of the ecological relationships of organisms. I have been unable to find any unambiguous references to the exclusion principle in the "Essays" of 1842 and 1844 (23); but in the *Origin* itself there are several passages that deserve recording. All the following passages are quoted from the sixth edition (11).

"As the species of the same genus usually have, though by no means invariably, much similarity in habits and constitution, and always in structure, the struggle will generally be more severe between them, if they come into competition with each other, than between the species of distinct genera. We see this in the recent extension over parts of the United States of one species of swallow having caused the decrease of another species. The recent increase of the missel-thrush in parts of Scotland has caused the decrease of the song-thrush. How frequently we hear of one species of rat taking the place of another species under the most different climates! In Russia the small Asiatic cockroach has everywhere driven before it its great congener. In Australia the imported hive-bee is rapidly exterminating the small, stingless native bee. One species of charlock has been known to supplant another species; and so in other cases. We can dimly see why the competition

should be most severe between allied forms, which fill nearly the same place in the economy of nature; but probably in no one case could we precisely say why one species has been victorious over another in the great battle of life" (p. 71).

"Owing to the high geometrical rate of increase of all organic beings, each area is already fully stocked with inhabitants; and it follows from this, that as the favored forms increase in number, so, generally, will the less favored decrease and become rare. Rarity, as geology tells us, is the precursor to extinction. We can see that any form which is represented by few individuals will run a good chance of utter extinction, during great fluctuations in the nature or the seasons, or from a temporary increase in the number of its enemies. But we may go further than this; for, as new forms are produced, unless we admit that specific forms can go on indefinitely increasing in number, many old forms must become extinct" (p. 102).

"From these several considerations I think it inevitably follows, that as new species in the course of time are formed through natural selection, others will become rarer and rarer, and finally extinct. The forms which stand in closest competition with those undergoing modification and improvement, will naturally suffer most. And we have seen in the chapter on the Struggle for Existence that it is the most closely-allied forms—varieties of the same species, and species of the same genus or related genera—which, from having nearly the same structure, constitution and habits, generally come into the severest competition with each other consequently, each new variety or species, during the progress of its formation, will generally press hardest on its nearest kindred, and tend to exterminate them. We see the same process of extermination among our domesticated productions, through the selection of improved forms by man. Many curious instances could be given showing how quickly new breeds of cattle, sheep and other animals, and varieties of flowers, take the place of older and inferior kinds. In Yorkshire, it is historically known that the ancient black cattle were displaced by the long-horns, and that these 'were swept away by the short-horns' (I quote the words of an agricultural writer) 'as if by some murderous pestilence'" (p. 103).

"For it should be remembered that

the competition will generally be most severe between those forms which are most nearly related to each other in habits, constitution and structure. Hence all the intermediate forms between the earlier and later states, that is between the less and more improved states of the same species, as well as the original parent species itself, will generally tend to become extinct" (p. 114).

Those passages are, we must admit, typically Darwinian; by turn clear, obscure, explicit, cryptic, suggestive, they have in them all the characteristics that litterateurs seek in James Joyce. The complexity of Darwin's work, however, is unintended; it is the result partly of his limitations as an analytical thinker, but in part also it is the consequence of the magnitude, importance, and intrinsic difficulty of the ideas he grappled with. Darwin was not one to impose premature clarity on his writings.

Origins in Economic Theory?

In chapter 3 of *Nature and Man's Fate* I have argued for the correctness of John Maynard Keynes' view that the biological principle of natural selection is just a vast generalization of Ricardian economics. The argument is based on the isomorphism of theoretical systems in the two fields of human thought. Now that we have at last brought the competitive exclusion principle out of the periphery of our vision into focus on the *fovea centralis* it is natural to wonder if this principle, too, originated in economic thought. I think it is possible. At any rate, there is a passage by the French mathematician J. Bertrand (24), published in 1883, which shows an appreciation of the exclusion principle as it applies to economic matters. The passage occurs in a review of a book of Cournot, published much earlier, in which Cournot discussed the outcome of a struggle between two merchants engaged in selling identical products to the public. Bertrand says: "Their interest would be to unite or at least to agree on a common price so as to extract from the body of customers the greatest possible receipts. But this solution is avoided by Cournot who supposes that one of the competitors will lower his price in order to attract the buyers to himself, and that the other, trying to regain them, will set his price still lower. The

two rivals will cease to pursue this path only when each has nothing more to gain by lowering his price.

"To this argument we make a peremptory objection. Given the hypothesis, no solution is possible: there is no limit to the lowering of the price. Whatever common price might be initially adopted, if one of the competitors were to lower the price unilaterally he would thereby attract the totality of the business to himself. . . ."

This passage clearly antedates Grinnell, Lack, *et al.*, but it comes long after the *Origin of Species*. Are there statements of the principle in the economic literature before Darwin? It would be nice to know. I have run across cryptic references to the work of Simonde de Sismondi (1773–1842) which imply that he had a glimpse of the exclusion principle, but I have not tracked them down. Perhaps some colleague in the history of economics will someday do so. If it is true that Sismondi understood the principle, this fact would add a nice touch to the interweaving of the history of ideas, for this famous Swiss economist was related to Emma Darwin by marriage; he plays a prominent role in the letters published under her name (25).

Utility of the Exclusion Principle

"The most important lesson to be learned from evolutionary theory," says Michael Scriven in a brilliant essay recently published (26), "is a negative one: the theory shows us what scientific explanations need not do. In particular it shows us that one cannot regard explanations as unsatisfactory when they are not such as to enable the event in question to have been predicted." The theory of evolution is not one with which we can predict exactly the future course of species formation and extinction; rather, the theory "explains" the past. Strangely enough, we take mental satisfaction in this ex post facto explanation. Scriven has done well in showing why we are satisfied.

Much of the theory of ecology fits Scriven's description of evolutionary theory. Told that two formerly separated species are to be introduced into the same environment and asked to predict exactly what will happen, we are generally unable to do so. We can only make certain predictions of this sort: either *A* will extinguish *B*, or *B* will extinguish *A*; or the two species

are (or must become) ecologically different—that is, they must come to occupy different ecological niches. The general rule may be stated in either of two different ways: *Complete competitors cannot coexist*—as was said earlier; or, *Ecological differentiation is the necessary condition for coexistence*.

It takes little imagination to see that the exclusion principle, to date stated explicitly only in ecological literature, has applications in many academic fields of study. I shall now point out some of these, showing how the principle has been used (mostly unconsciously) in the past, and predicting some of its applications in the future.

Economics. The principle unquestionably plays an indispensable role in almost all economic thinking, though it is seldom explicitly stated. Any competitor knows that unrestrained competition will ultimately result in but one victor. If he is confident that he is that one, he may plump for "rugged individualism." If, on the other hand, he has doubts, then he will seek to restrain or restrict competition. He can restrain it by forming a cartel with his competitors, or by maneuvering the passage of "fair trade" laws. (Laboring men achieve a similar end—though the problem is somewhat different—by the formation of unions and the passage of minimum wage laws.) Or he may restrict competition by "ecological differentiation," by putting out a slightly different product (aided by restrictive patent and copyright laws). All this may be regarded as individualistic action.

Society as a whole may take action. The end of unrestricted competition is a monopoly. It is well known that monopoly breeds power which acts to insure and extend the monopoly; the system has "positive feedback" and hence is always a threat to those aspects of society still "outside" the monopoly. For this reason, men may, in the interest of "society" (rather than of themselves as individual competitors), band together to insure continued competition; this they do by passing anti-monopoly laws which prevent competition from proceeding to its "naturally" inevitable conclusion. Or "society" may permit monopolies but seek to remove the power element by the "socialization" of the monopoly (expropriation or regulation).

In their actions both as individuals and as groups, men show that they have an implicit understanding of the

exclusion principle. But the failure to bring this understanding to the level of consciousness has undoubtedly contributed to the accusations of bad faith ("exploiter of the masses," "profiteer," "nihilist," "communist") that have characterized many of the interchanges between competing groups of society during the last century. F. A. Lange (27), thinking only of laboring men, spoke in most fervent terms of the necessity of waging a "struggle against the struggle for existence"—that is, a struggle against the unimpeded working out of the exclusion principle. Groups with interests opposed to those of "labor" are equally passionate about the same cause, though the examples they have in mind are different.

At the present time, one of the great fields of economics in which the application of the exclusion principle is resisted is international competition (nonbellicose). For emotional reasons, most discussion of problems in this field is restricted by the assumption (largely implicit) that Cournot's solution of the *intranational* competition problem is correct and applicable to the *international* problem. On the less frequent occasions when it is recognized that Bertrand's, not Cournot's, reasoning is correct, it is assumed that the consequences of the exclusion principle can be indefinitely postponed by a rapid and endless multiplication of "ecological niches" (largely unprotected though they are by copyright and patent). If some of these assumptions prove to be unrealistic, the presently fashionable stance toward tariffs and other restrictions of international competition will have to be modified.

Genetics. The application of the exclusion principle to genetics is direct and undeniable. The system of discrete alleles at the same gene locus competing for existence within a single population of organisms is perfectly isomorphic with the system of different species of organisms competing for existence in the same habitat and ecological niche. The consequences of this have frequently been acknowledged, usually implicitly, at least since J. B. S. Haldane's work of 1924 (28). But in this field, also, the consequences have often been denied, explicitly or otherwise, and again for emotional reasons. The denial has most often been coupled with a "denial" (in the psychological sense) of the priority of the inequality axiom. As a result of recent findings in the fields of physiological

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genetics and population genetics, particularly as concerns blood groups, the applicability of both the inequality axiom and the exclusion principle is rapidly becoming accepted. William C. Boyd has recorded, in a dramatic way (29), his escape from the bondage of psychological denial. The emotional restrictions of rational discussion in this field are immense. How "the struggle against the struggle for existence" will be waged in the field of human genetics promises to make the next decade of study one of the most exciting of man's attempts to accept the implications of scientific knowledge.

Ecology. Once one has absorbed the competitive exclusion principle into one's thinking it is curious to note how one of the most popular problems of evolutionary speculation is turned upside down. Probably most people, when first taking in the picture of historical evolution, are astounded at the number of species of plants and animals that have become extinct. From Simpson's gallant "guesstimates" (30), it would appear that from 99 to 99.975 percent of all species evolved are now extinct, the larger percentage corresponding to 3999 million species. This seems like a lot. Yet it is even more remarkable that there should live at any one time (for example, the present) as many as a million species, more or less competing with each other. Competition is avoided between some of the species that coexist in time by separation in space. In addition, however, there are many ecologically more or less similar species that coexist. Their continued existence is a thing to wonder at and to study. As Darwin said (11, p. 363)—and this is one

more bit of evidence that he appreciated the exclusion principle—"We need not marvel at extinction; if we must marvel, let it be at our own presumption in imagining for a moment that we understand the many complex contingencies on which the existence of each species depends."

I think it is not too much to say that in the history of ecology—which in the broadest sense includes the science of economics and the study of population genetics—we stand at the threshold of a renaissance of understanding, a renaissance made possible by the explicit acceptance of the competitive exclusion principle. This principle, like much of the essential theory of evolution, has (I think) long been psychologically denied, as the penetrating study of Morse Peckham (31) indicates. The reason for the denial is the usual one: admission of the principle to consciousness is painful. [Evidence for such an assertion is, in the nature of the case, difficult to find, but for a single clear-cut example see the letter by Krogman (32).] It is not sadism or masochism that makes us urge that the denial be brought to an end. Rather, it is a love of the reality principle, and recognition that only those truths that are admitted to the conscious mind are available for use in making sense of the world. To assert the truth of the competitive exclusion principle is not to say that nature is and always must be, everywhere, "red in tooth and claw." Rather, it is to point out that every instance of apparent coexistence must be accounted for. Out of the study of all such instances will come a fuller knowledge of the many prosthetic devices of co-

existence, each with its own costs and its own benefits. On such a foundation we may set about the task of establishing a science of ecological engineering.

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Science in the News

Enforcing an Atom Test Ban: Scientists Testify Before Joint Atomic Energy Committee

The scientific testimony before the Joint Congressional Atomic Energy Committee last week left no doubt that enormous technical problems are involved in establishing a reliable system for detecting underground atomic explosions.

This came as no surprise to the committee, which made it clear at the outset that the purpose of the inquiry was not to develop new information, but to bring the available information more forcefully to the public's attention than had been done so far. The committee, the Atomic Energy Commission, the Defense Department, and some scientists have strong reservations about the Administration's efforts to negotiate a test ban with the Russians. It was no secret that the committee's purpose was to win support for their pessimistic view of the situation.

Leading scientists, including some who favor the test ban, were put on record as convinced that the proposed "Geneva system" (a network of 180 seismographic stations spaced around the globe) is, on technical grounds anyway, hopelessly inadequate as a reliable system for enforcing a ban on all tests.

Indeed, there was general agreement that even a greatly expanded network of detection stations would not guarantee that even fairly large tests, involving blasts the strength of the Hiroshima bomb or larger, would be identified and located. It was concern over these difficulties that led to the American proposal of 11 February to limit the ban to explosions above a certain threshold.

The Russians, in turn, on 19 March announced their willingness to accept the American proposal with the condition that the U.S., Great Britain, and

the Soviet Union agree to an unpoliced moratorium on underground explosions below the threshold. During the moratorium both East and West would conduct research to improve methods of detection. Until this reply to the American proposal, the Soviets have been insisting that a treaty must ban all nuclear explosions: in the atmosphere, in outer space, under ground, and under water.

Muffling the Explosions

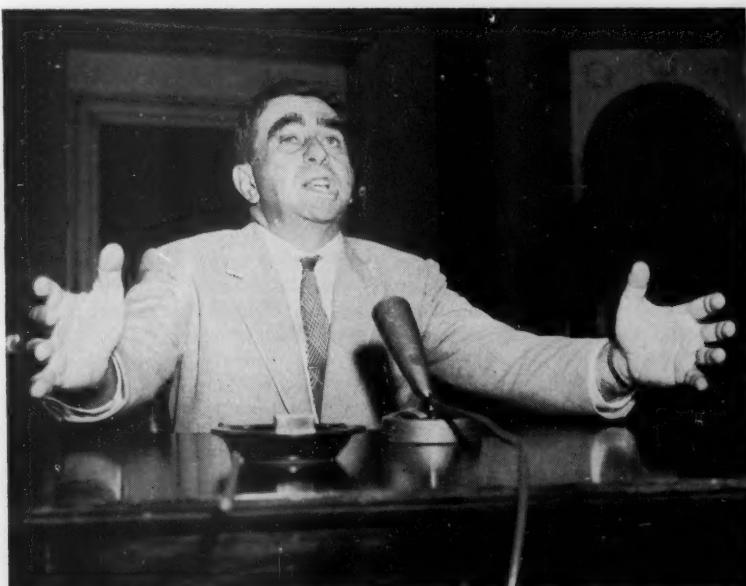
The most discussed of the many technical difficulties were the techniques, brought to the attention of the Russians last year, for "decoupling" (muffling) an explosion. This would make tests far more difficult for seismographs to detect than had been considered likely at the 1958 conference of technical experts

at which the Geneva system was worked out.

The committee heard convincing testimony that the so-called "big hole" theory is not only sound, but practical. Large underground chambers, the scientists said, could muffle explosions by a factor of as much as 300. Thus, a fully muffled 100 kiloton hydrogen explosion (equivalent to 100,000 tons of TNT) would pass completely unnoticed if the 1958 Geneva network were in use.

The committee was told that in this country alone there are several hundred man-made and natural underground chambers that can be used to muffle small tests by factors of from 30 to 300 times.

The entire 1958 Geneva system, it seems, was based on the incorrect assumption that a few tests in Nevada gave a reasonably accurate basis for estimating how well the shock waves from an explosion would register on distant seismographs. But it was pointed out that explosions could be decoupled by a factor of 3 by doing nothing more than setting the blast off in harder rock than that found in the rather resonant Nevada strata. This means that nations would have to go to virtually no trouble or expense at all to muffle a 60 kiloton blast (three times the force of



Edward Teller, "Father of the H-bomb," gestures during his appearance before the Joint Atomic Energy Committee. Teller, the leading scientific opponent of a test ban, told the committee that tests of "tactical" nuclear weapons—and some large ones—could escape detection now and for some years to come. [United Press]

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A Missing Factor

The weakness of the hearings was brought into the open with the unexpected appearance of Nobel Prize Winner Harold Urey at the panel discussion the committee held among the scientists on the third morning of the hearings.

Even before Urey spoke, suggestions arose that the network of seismic stations might not be the only source of information on clandestine tests. Seismologist Roland Beers told an allegory about a mining operator who was unable to find a profitable strike despite a tremendous investment in the latest scientific equipment. He found what he was after, nevertheless, because one day a lucky old prospector wandered into his office and offered to show him what he was looking for.

Senator Wallace Bennett (R-Vt.) amplified this by noting that the story reminded him of the bridge motto, "one peek is worth two finesse."

But it was not until Urey got the chance to speak that anyone, in so many words, expressed the opinion that, granting the weaknesses of the proposed network of seismic stations, it would be extremely difficult for the Russians or anyone else to hold atomic tests without some rival power's intelligence system getting wind of them.

So, although the committee succeeded in putting on record a convincing picture of the technical difficulties of detecting underground tests, the question of how great a risk there is of the Russians' actually carrying on testing after a ban remained essentially unanswered.

And the ultimate question of how great a risk the U.S. should be willing to undertake in return for the various advantages of a test ban was not dealt with at all.

The Drug Hearings:

Kefauver Continues His Campaign

Senator Kefauver's lengthy exposé of the drug industry, now in its fifth month, continues to roll along.

Since December the senator has looked into the production and marketing of steroid hormones and tranquilizers, with time out to listen to critics, and occasionally defenders, of the indus-

try at large. This week he was concentrating his attention on oral antidiabetics. Next month he plans to set to work on antibiotics.

The hearings have certainly been politically useful to Kefauver, who is up for re-election this year, and it can be assumed that they will be arranged to reach some sort of climax in June or July, whenever the senator feels the publicity will help him most in his 4 August primary, tantamount to election in Tennessee.

But, conceding this political usefulness, it is still difficult to dismiss the investigation as nothing more than an elaborate publicity stunt. Kefauver has won the support of the people who would normally have little in common with his politics, including, for example, such an eminent and widely respected exponent of free enterprise as Sen. Frank Lausche of Ohio. And, as a result of the hearings, the industry has drawn the critical attention of several of the leading magazines, beginning with a piece in *Life* ["Big pill to swallow" (15 Feb. 1960)].

A measure of the industry's uneasiness was indicated when *F-D-C Reports* ("The Pink Sheet"), a confidential Washington newsletter serving the drug and cosmetics industries, took the unusual step of offering its subscribers a daily report on what Kefauver is doing. A good part of the weekly newsletter's space, recently, has been devoted to what it calls the "fallout" from the Kefauver hearings, most notably the empaneling of a grand jury in New York to look for antitrust violations within the industry. The grand jury investigation, according to the *New York Times*, "stems from testimony given in recent hearings before the Senate Subcommittee on Antitrust and Monopoly" (that is, from the Kefauver Committee).

A One Man Show

The show is entirely Kefauver's. The senator, his hair now speckled with gray, is generally the only one of the eight committee members present at the hearings. He sits, virtually alone at the long committee table, a white knight supported by the committee's husky chief council, Paul Dixon, sitting at his right. Dixon asks most of the questions, with Kefauver stepping in occasionally, almost always to the discomfort of the witness if he is from the industry. (Representatives of the industry at large, or of individual companies, are

clearly regarded as enemy, from whom the truth must be torn.)

Profits and Promotion

Except for possible antitrust violations, and even the most reputable companies occasionally run afoul of these laws, no one has suggested any serious wrong-doing on the part of the drug companies. The basic issue, rarely stated clearly by either side, seems to be whether the industry should be allowed to run itself as a normal business, or whether its special position justifies the federal government's taking steps to see that it is run as a public service.

The leading companies stand accused by the committee of making excessive profits (fourth highest among American industries, more than double the 11 percent average of all industries); of spending most of their heavy investment in research on studies that are of commercial rather than scientific value (that is, of putting most of their effort into developing profitable variations of available drugs as opposed to developing really new medicines); and of brainwashing the physicians by spending enormous amounts of money on promotion.

To take full advantage of their promotional effort and of their carefully cultivated, and normally thoroughly deserved, reputation for excellent quality control, the companies use a peculiar system of branding which successfully encourages doctors to write their prescriptions using individual company's trade name for a drug rather than the generic name. Few nonmedical readers would recognize a drug called meprobamate. But almost everyone has heard of Miltown and Equinil, which are the trade names under which Carter, the patent holder, and Wyeth, a licensee, sell meprobamate.

The public ends by paying, according to testimony before the committee, often three times or more money for a prescription specifying the trade name of a product than it would pay for the same prescription specifying only the generic name. In the case of patentable medicines, the price would be the same for the medicine under either the generic or trade name, since even if the patent holder licenses other companies to make the medicine there usually seems to be a tacit agreement to charge the same (high) price.

Presumably some lessening in the physicians' tendency to prescribe by

trade name will come as a result of the publicity generated by the hearings and the magazine articles. A modest move in this direction was even made by the AMA at its last convention, when a resolution was passed suggesting that doctors use generic names when prescribing for indigent patients.

A number of suggestions of what should be done have been made before the committee. The two that seem most likely to drastically change the situation are these:

1) Passage of a law giving the government effective supervision over drug manufacture, similar to that which has existed for many years in the meat packing industry. This, proponents say, would make doctors much more willing to prescribe by generic rather than brand name, since they would no longer have to rely on the specific company's reputation as the only clear guarantee of the potency and safety of its products.

2) Establishment of a program, perhaps run jointly by the Food and Drug Administration and the American Medical Association, to keep physicians informed on the relative value and price of new drugs. As things are now the physicians have no convenient index of information that would allow them to sort out the misleading from the meaningful messages among the barrage of promotion to which they are subject (about a pound of mail a day plus regular visits from the companies' "detail men").

The idea behind these and similar proposals is that they would bring about a decline in the purportedly excessive profits, pseudo research, and promotion, since the economic situation that brought these things into existence would be sharply altered.

There is not enough time left in this session of Congress to push through any strong legislation, even if Kefauver should offer such proposals, which he has not yet done.

Whatever legislation is offered, this year or later, will have to face determined opposition from the industry, probably supported by the American Medical Association, which has always worked very closely with the drug industry on legislative matters. Testifying before the committee last week, Austin Smith, president of the Pharmaceutical Manufacturers Association, never sounded more confident than when he assured Kefauver that when representatives of the AMA were called as wit-

nesses they would endorse the industry's point of view.

In the past the AMA has tended to regard almost any increase in the government's activity in the medical field as another step on the road to socialized medicine. And the ultimate lines in this controversy can most usefully be drawn not between those who think drug prices are too high and those who do not, but between those who would be willing to see a substantial increase of federal activity in the medical field and those who are not so willing.

Regulations for Selection of Fulbright Scholars Changed

Last year's public concern about the standards and procedures for the selection of Fulbright scholars has resulted in significant changes in the rules promulgated by the President's Board of Foreign Scholarships. The regulations which caused difficulty and those that have replaced them are discussed in an article by Louis Joughin of the staff of the American Association of University Professors that appears in the spring issue of the *AAUP Bulletin*.

The new regulations provide that all evidence relating to the possible disloyalty of a candidate shall be turned over to law enforcement agencies for treatment similar to that given evidence relating to any other kind of possible felony. The board has thus denied itself opportunity to make informal inquiry about disloyalty and to apply, in this area, vague standards without responsibility under law.

Another procedural innovation relates to rejections, by the board's subcommittee on appointments, of candidates approved by the screening committees of the Conference Board of Associated Research Councils, who make the actual nominations. Henceforth a reversal of this sort will automatically result in review by the whole Board of Foreign Scholarships to consider all the facts.

Last year's rejection of Darwin specialist Bert Loewenberg of Sarah Lawrence College for a Fulbright lecture-ship particularly disturbed some of this country's scholars because his application had been highly endorsed and because a request had been received from scholars of Leeds University, in England, for his services during the Darwin centennial year. The Board of Foreign Scholarships, in response to public

inquiry, said that disloyalty had not been a factor in its decision but gave no other explanation.

The article in the *AAUP Bulletin* points out three problems which have not been fully solved. First, the Board of Foreign Scholarships remains free to select candidates on the basis of their "potential contribution to the objectives of the program," as set forth in the board's policy statements. This vague standard permits the consideration of any kind of evidence that the board may regard as relevant, including evidence which is not academic, and even including some which might relate to "loyalty."

Second, the board continues to reserve the right to consider secret evidence which the screening committees have not seen. Third, since the function of the board is to make decisions about scholarly matters, it would seem desirable that its membership consist chiefly of persons who qualify in the first instance as distinguished scholars in the several fields of learning; this has not recently been the case, Joughin says.

In commenting on the situation, Joughin points out that his article could not have been written without the cooperation of officials in the State Department and the Conference Board of Associated Research Councils, who made possible full and frank criticism of the program they administer.

AAAS Socio-Psychological Prize

Through the generosity of an anonymous donor, the AAAS offers an annual prize of \$1000 for a meritorious essay in socio-psychological inquiry. Previous winners of this prize and the titles of their essays have been: Arnold M. Rose, "A theory of social organization and disorganization"; Yehudi A. Cohen, "Food and its vicissitudes: a cross-cultural study of sharing and non-sharing in sixty folk societies"; Herbert C. Kelman, "Compliance, identification, and internalization: a theoretical and experimental approach to the study of social influence"; Irving A. Taylor, "Similarities in the structure of extreme social attitudes"; and Stanley Schachter, "The psychology of affiliation."

Conditions of Competition

The conditions of competition for the prize to be awarded at the 1960 annual meeting, New York City, 26-31 December, are as follows:

- 1) The contribution should further

the comprehension of the psychological-social-cultural behavior of human beings—the relationships of these hyphenated words being an essential part of the inquiry. Whether the contributor considers himself to be an anthropologist, a psychologist, a sociologist, or a member of some other group is unimportant as long as his essay deals with basic observation and construction in the area variously known as social process, group behavior, or interpersonal behavior. For ease of reference in the rest of this statement, this general area will be called "social behavior."

2) The prize is offered to encourage studies and analyses of social behavior based on explicitly stated assumptions or postulates, which lead to experimentally verifiable conclusions or deductions. In other words, it is a prize intended to encourage in social inquiry the development and application of dependable methodology analogous to the methods that have proved so fruitful in the natural sciences. This is not to state that the methods of any of the natural sciences are to be transferred without change to the study of social behavior, but rather that the development of a science of social behavior is fostered through observation guided by explicit postulates, which in turn are firmly grounded on prior observations. It may be taken for granted that such postulates will include a spatial-temporal framework for the inquiry. It may properly be added that the essay should foster liberation from philosophic-academic conventions and from dogmatic boundaries between different disciplines.

3) Hitherto unpublished manuscripts are eligible, as are manuscripts that have been published since 1 January 1959. Entries may be of any length, but each should present a completed analysis of a problem, the relevant data, and an interpretation of the data in terms of the postulates with which the study began. Preference will be given to manuscripts not over 50,000 words in length. Entries may be submitted by the author himself or by another person on his behalf.

4) Entries will be judged by a committee of three persons considered well qualified to judge material in this field. The judges will be selected by a management committee consisting of the chairman and the secretary of Section K and the executive officer of the AAAS. The committee of judges reserves the right to withhold the prize if no worthy essay is submitted.

5) Entries should be sent to Dael Wolfe, Executive Officer, American Association for the Advancement of Science, 1515 Massachusetts Ave., NW, Washington 5, D.C. Entries should be submitted in quadruplicate. Each entry should be accompanied by six copies of an abstract not to exceed 1200 words in length. The name of the author should not appear anywhere on the entry itself but should be enclosed on a separate sheet of paper which also gives the author's address and the title of his essay. Entrants who wish to have their manuscripts returned should include a note to that effect and the necessary postage. In order to be eligible for consideration for the prize that will be awarded at the 1960 annual meeting of the Association, entries must be received *not later than 1 September 1960*.

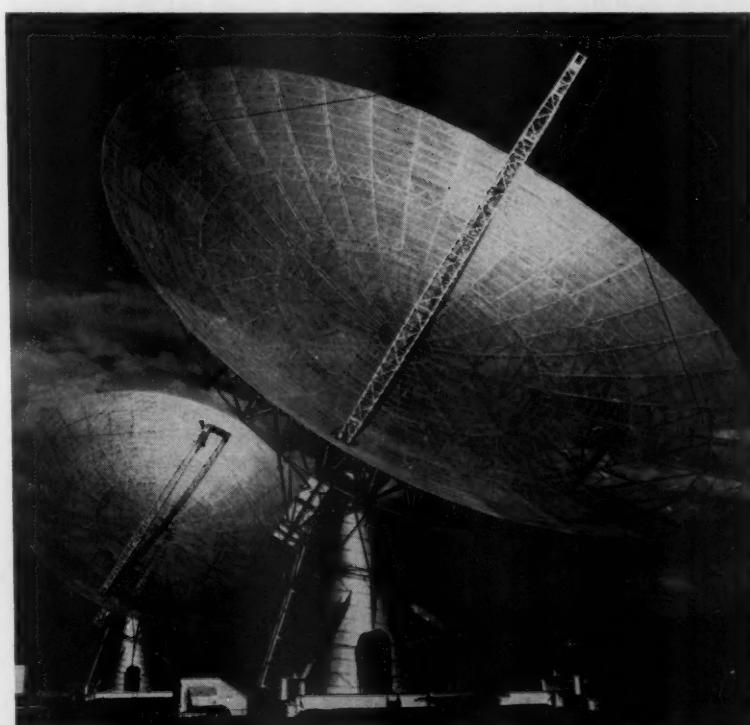
Navy Telescope Locates New Radio Sources in Space

Nine radio sources from outside Earth's galaxy have been located in the first 2 months of operation of a new radio telescope that is run for the Navy

by California Institute of Technology. Until the end of 1959, when the telescope went into full operation, only five other radio sources of the more than 100 detected outside the Milky Way had been precisely located and identified by all the radio telescopes of the world. The giant instrument was built by CIT under contract with the Office of Naval Research.

Located in a valley 259 miles from Los Angeles, the facility consists of two 90-foot parabola antennas mounted on a 1600-foot-long railroad track running east and west. Working in tandem, the twin dishes have a resolving power greater than that of any other known radio telescope in operation or under construction.

Although at present the radio telescope is being used to try to identify other radio sources in space, the two antennas can also be used to study planets and other large celestial bodies. Two planets can be studied at the same time by using the dishes independently. It is hoped that the new instrument may be helpful in determining whether the planet Jupiter is surrounded by a corona or by a Van Allen type radiation belt.



The new twin-antenna radio telescope that is operated for the Navy by California Institute of Technology. [U.S. Navy]

New Guinea Expedition Returns with 76,000 Specimens

The Sixth Archbold Expedition to New Guinea has returned to the American Museum of Natural History after 8 months of field research, which included the most extensive biological survey ever undertaken on Mount Wilhelm, highest mountain in eastern New Guinea. The return of Leonard J. Brass, associate curator in the museum's department of mammals and leader and botanist of the expedition, and Hobart M. Van Deusen, assistant curator and expedition mammalogist, preceded by a few days the arrival of 17 packing cases and crates containing approximately 76,000 animal and plant specimens collected in the Territory of New Guinea, northeast sector of the main island.

The total count of specimens is broken down as follows: 2295 mammals, 50 birds, 4105 amphibians and reptiles, about 50,000 insects and spiders, and 18,271 plants. In addition, approximately 1500 ectoparasites of mammals were collected for the research department of the Army Medical Service. The botanical specimens are being turned over to the United States National Herbarium in Washington for further study.

The expedition, like its five predecessors in New Guinea and one in Australia, was sponsored by Richard Archbold, research associate of the museum and president of Archbold Expeditions, a nonprofit corporation affiliated with the museum. The present project was also financed in part by a grant to Brass from the National Science Foundation and a grant from the Explorers Club to Van Deusen. The over-all aim of the Archbold Expeditions is to shed light on the geographical and ecological relationships of the animal and plant life of the New Guinea-Australia area.

Nutrition of Chileans Studied by United States Team

In response to a request from the Government of Chile, a team of U.S. experts began, on 28 March, a 70-day survey of the nutritional status of the Chilean population. The purpose of the study is to assess the current standards and needs of the people, to formulate practical recommendations for improvement consistent with the country's resources, and to provide technical assistance for the development of stand-

ard ration requirements and local nutrition services.

The survey was arranged by the Interdepartmental Committee on Nutrition for National Defense, which operates administratively through the National Institute of Arthritis and Metabolic Diseases, National Institutes of Health. Team members include physicians, biochemists, food technologists, and others, who are working with Chilean personnel during the survey.

Large numbers of individuals from the Chilean Armed Forces and selected civilian groups will be examined during the study. Laboratory equipment and supplies, which have already been shipped to Santiago, will be turned over to the Chilean Government under Mutual Assistance Program procedures upon completion of the survey.

The director of the 15-member team is William F. Ashe, Jr., head of the department of preventive medicine, Ohio State University. Deputy survey director and laboratory director is Carl A. Baumann, department of biochemistry, University of Wisconsin College of Agriculture.

Nutrition surveys have been completed by the Interdepartmental Committee on Nutrition for National Defense in Alaska and in 11 countries: Korea, Iran, Pakistan, the Philippines, Turkey, Libya, Spain, Ethiopia, Peru, Ecuador, and Viet Nam. Future surveys are scheduled to be made in Columbia (May 1960), Formosa (September 1960), Thailand (October 1960), and Lebanon (March 1961).

Moscow Polio Conference Inaugurates Exchange Agreement

Four American scientists have been designated so far to represent the United States at a joint American-Soviet meeting on poliomyelitis to be held in the Soviet Union in May. The formal invitation to send an American delegation to the meeting was extended to Surgeon General Leroy E. Burney of the Public Health Service by Sergei Kurashov, Minister of Health of the Union of Soviet Socialist Republics, in accordance with the U.S.-U.S.S.R. Exchange Agreement of 21 November 1959.

The mission is the first to take place under the agreement. The agreement, in addition to providing for joint meetings on poliomyelitis, cancer, and heart disease, calls for exchange missions between the two countries on scientific

matters and authorizes the Surgeon General of the Public Health Service and the Soviet Minister of Health to develop cooperative projects in scientific and medical areas of joint interest.

The poliomyelitis meeting will be held at the Poliomyelitis Institute of the Academy of Medical Sciences in Moscow from 12 to 16 May. A similar joint meeting will be held in the United States next year.

The personal representative of the Surgeon General and chairman of the United States delegation will be David E. Price, Assistant Surgeon General of the Public Health Service. The three other members of the delegation that have been named are Roderick Murray, of the service's National Institutes of Health; Alexander Langmuir, of the PHS Communicable Disease Center, Atlanta, Ga.; and Albert Sabin of Children's Hospital, Research Foundation, Cincinnati, Ohio.

Foundation Sponsors Special Physics Institute at Wisconsin

A special Summer Institute for Theoretical Physics will be held at the University of Wisconsin, 20 June-26 August, with support from the National Science Foundation. The purpose of the institute is to bring together in a relaxed and informal atmosphere a number of theoretical physicists having mutual interests. The principal activities of the institute will consist of informal discussions and independent research. There will be no courses, but each invited member will present a lecture or two concerning his current research.

Invited members, who will be in residence for a period of 3 weeks or more, include the following: P. Federbush, Massachusetts Institute of Technology; R. Haag, University of Illinois; K. Johnson, Massachusetts Institute of Technology; H. Lehmann, University of Hamburg; P. T. Matthews, Imperial College, London; L. Michel, University of Paris; A. Salam, Imperial College, London; and J. C. Ward, Carnegie Institute of Technology. Theoretical physicists on the staff of the University of Wisconsin will also join in the activities of the institute.

Participation will be open to qualified theoretical physicists. Limited funds are available for travel support for those attending. Inquiries and applications for support may be sent to: Summer Institute for Theoretical Physics, Sterling Hall, University of Wisconsin.

News Briefs

Messages from other worlds? This country's first systematic search for signals from intelligent beings on other planets began on 11 April with the start of Project Ozma, directed by Frank D. Drake of the National Radio Astronomy Observatory, Green Bank, W. Va. The National Science Foundation reports that the 85-foot Howard Tatel memorial telescope will be used for the search, for as much as 6 hours each day. The two stars in our galaxy that are nearest the sun and that might have planets susceptible of sustaining life—Tau Ceta and Epsilon Eridani—are being observed during a month-long investigation. The research involves listening for signals. No facilities for transmitting signals are available.

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UNESCO arid lands meeting. A symposium on arid lands research will be held in Paris at UNESCO headquarters, 11–18 May. The UNESCO Advisory Committee on Arid Zone Research has recommended that the symposium be divided into the following parts: (i) critical appraisal of the state of knowledge; (ii) appraisal of UNESCO's activities; (iii) studies on selected problems illustrating difficulties in translating knowledge into action in arid zone development; and (iv) study of the prospects for future international and national action.

* * *

Balloon-type satellite. An attempt will be made to launch a 100-foot inflatable sphere into orbit 1000 miles above the earth on 5 May, according to an announcement by the National Aeronautics and Space Administration. The communications experiment, Project Echo, has been scheduled so that the balloon-like sphere, made of highly reflective aluminum, will remain in continuous sunlight for about 2 weeks. No radio tracking beacon will be attached to the sphere. The third stage of the launching vehicle will carry a transmitter which will broadcast on 108.06 Mcy/sec for 8 or 10 days, the expected lifetime of its batteries.

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Diploma mills. The Department of Health, Education, and Welfare has announced that it has started to compile a permanent list of "diploma mills" in this country as a warning not only to the public but also to gullible persons in other countries. Eleven of the 30 institutions named were found to be active in selling college degrees over-

seas. A "diploma mill" was defined as "an organization that awards degrees without requiring its students to meet educational standards for such degrees established and traditionally followed by reputable national institutions."

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Locust control. A cooperative drive to combat the desert locust began this month when 13 countries signed an agreement at the headquarters of the United Nations Food and Agriculture Organization, putting in effect a 6-year project for locust control at a cost of more than \$3,800,000. Nineteen governments are cooperating in the initial stage, and others may join later. Beginning in May, locust swarms will be attacked by land and air in a large belt stretching from Morocco on the Atlantic Ocean to the Himalayas. The United Nations Special Fund, a new agency for long-range aid to less developed areas, is providing \$2,492,700 for the work. This is the largest contribution the agency has made to any individual project since it started operations 18 months ago.

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Univac programs convention. The Federation of American Societies for Experimental Biology used Remington Rand's Univac to program its 1960 convention, which was held in Chicago, 11–15 April. With the help of the machine, the 421-page abstract index, with its 2526 abstracts, was programmed in 2 minutes. The entire project required only 8 hours of Univac's time, whereas in the past as many as 1200 man-hours have been devoted to the task.

* * *

Horticulture society. The International Society for Horticultural Science was formed at The Hague on 26 March. Eighteen member countries were represented at the meeting. The society will sponsor international gatherings on scientific horticulture, publish an information bulletin at irregular intervals, and publish the proceedings of international meetings. The next international congress will be held at Brussels in early September 1962. Officers of the society are A. Lecrenier of Belgium, president; H. B. Tukey, Michigan State University, vice-president; and G. de-Bakker, the Netherlands, secretary-treasurer.

* * *

Scintillator conference. An international Conference on Organic Scintillation Detectors will be held at the University of New Mexico, 15–17 August. The program, which is under the chair-

manship of E. Newton Hayes of the Los Alamos Scientific Laboratory, will include a review of fundamentals and discussion of new advances and applications in biology, medicine, chemistry, physics, and industry. The general chairman of the conference is Guido H. Daub, Chemistry Department, University of New Mexico, Albuquerque.

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Tropical dermatology. A meeting to organize the International Society of Tropical Dermatology was held in January at the New York Academy of Medicine. Aldo Castellani of the Instituto de Medicina Tropical, Lisbon, was elected president. The inaugural meeting is scheduled for the evening of 10 May at the Rockefeller Institute in New York. Castellani will be the principal speaker. For information, write to the secretary-general, Frederick Reiss, 870 5th Ave., New York 21, N.Y.

* * *

Soviet air-pollution research. U.S.S.R. *Literature on Air Pollution and Related Occupational Diseases*, the first volume of a survey that is being conducted by Ben S. Levine under a U.S. Public Health Service grant, has been released for sale to the public by the Office of Technical Services, Business and Defense Services Administration, U.S. Department of Commerce. Levine's work is primarily for the benefit of the English-speaking scientist who wants information on Russian air-pollution research. The survey covers the basic concepts of air-pollution prevention in the U.S.S.R., explains the methods of approach, outlines the scope and plan of the work, and evaluates the progress made.

* * *

Name changes at NYU. By joint action, the Board of Trustees of New York University and the NYU Medical Center Board have effected the following name changes. The name of the Medical Center is now: New York University Medical Center (formerly New York University–Bellevue Medical Center). The name of the College of Medicine has been changed to: New York University School of Medicine (formerly New York University College of Medicine). The center's affiliation with the Bellevue center will continue unchanged. The elimination of "Bellevue" in the center's title is to emphasize the fact that the center is an integral part of New York University, a private institution, rather than one which is municipally or state owned.

Scientists in the News

Presentation of the National Academy of Sciences medals took place on 25 April in Washington during the academy's 97th annual meeting.

Anton F. Bruun, Zoological Museum of the University of Copenhagen, received the Agassiz Medal for original contribution in the science of oceanography.

Alfred S. Romer, Museum of Comparative Zoology at Harvard College, received the Daniel Giraud Elliot Medal, given for the most meritorious work in zoology or paleontology published each year.

George W. Beadle, California Institute of Technology, received the Kimber Genetics Medal for achievement in the science of genetics.

Ernst J. Opik, University of Maryland and Armagh Observatory (Ireland), received the J. Lawrence Smith Medal for investigations of meteoric bodies.

Yusuke Hagihara, University of Tokyo, received the James Craig Watson Medal, given for noteworthy astronomical discoveries or research.

John R. Heller, director of the National Cancer Institute, Bethesda, Md., since 1948, has been named president and executive officer of the Memorial Sloan-Kettering Cancer Center, New York, effective 1 July. The center is to be a new corporate body composed of the present Memorial Center for Cancer and Allied Diseases and the Sloan-Kettering Institute for Cancer Research, which are now operated under a joint committee.



John R. Heller

Lowell T. Coggeshall, dean of the division of biological sciences of the University of Chicago, has been named a vice president of the university. He will be responsible primarily for the development of medical research programs and facilities.

Edward J. Rhoad, project manager of Frescanar, an advanced radar development for the U.S. Army, has been named senior scientist at Hughes Aircraft Company's ground systems group, Fullerton, Calif.

John H. Litchfield, assistant professor of food engineering at Illinois Institute of Technology, now heads research studies in food technology and fermentation in the biosciences division of Battelle Memorial Institute, Columbus, Ohio.

Margaret Mead, associate curator of ethnology at the American Museum of Natural History and adjunct professor of anthropology at Columbia University, delivered the annual Gay Lecture on Medical Ethics at the Harvard Medical School, 25 April. She discussed "New Complexities Accompanying New Theories."

Darrell A. Russel, assistant professor of soil chemistry at the North Louisiana Hill Farm Experiment Station, has been named agriculturist in the fertilizer distribution branch of the Tennessee Valley Authority.

Ira R. Telford, professor of anatomy at George Washington University (Washington, D.C.), left in March for a 6-month exchange visit to the University of St. Andrews, Fife, Scotland. **Robert Walmsley** of St. Andrews is now at George Washington's School of Medicine as visiting professor of anatomy.

Gerold H. Tenney, leader of the nondestructive testing group at the Los Alamos Scientific Laboratory, was awarded the Scroll of Appreciation and gold medal of the third international Conference on Nondestructive Testing, held in Tokyo in March.

Glen Wade, associate professor of electrical engineering at Stanford University, has been named associate director of engineering for general research for the microwave and power tube division of Raytheon Co., Burlington, Mass.

Recent Deaths

Frank D. Carvin, Summit, N.J.; 67, former director of the department of engineering at Illinois Institute of Technology; author of *Propulsion of Land, Air and Water Vehicles*; specialist in molecular physics and spectra analysis; 24 Mar.

William W. Christmas, New York, N.Y.; 94; pioneer in aeronautics, who held patents on 300 inventions; 14 Apr.

Fred C. Croxton, Columbus, Ohio; 88; well-known labor conciliator for the U.S. Department of Labor, who was a fellow of the AAAS for 54 years; 3 Apr.

Franklin S. Harris, Salt Lake City, Utah; 75; agricultural scientist; president of Brigham Young University from 1921 to 1945, then president of Utah State Agricultural College, which he left in 1950 to head an agricultural mission to Iran under the auspices of the United Nations Food and Agricultural Organization; 18 Apr.

Harold J. Lockwood, Hartford, Conn.; 69; Hallden professor of engineering at Trinity College; joined Trinity as engineering chairman in 1943; professor of engineering at Manhattan College, 1938-42, and at Dartmouth, 1921-32; 15 Apr.

Tao Meng-ho, Shanghai; 73; leading Chinese sociologist and author; dean of the National Peiping University from 1914 to 1917; under the Communist regime served as vice president of the Academy of Sciences and became deputy to the National People's Congress, Communist China's Parliament; 17 Apr.

William W. Morrison, New York, N.Y.; 65; otolaryngologist who in 1936 demonstrated a method of articulation by which a patient who had lost his larynx because of throat cancer could speak; former associate clinical professor in the Post-Graduate School of Medicine of New York University; 16 Apr.

John D. Sherman, Jr., Mount Vernon, N.Y.; 87; entomologist and dealer in scientific books and magazines; 17 Apr.

Bogumil M. Wojciechowski, New York, N.Y.; 53; engineer at the Bell Telephone Laboratories in Murray Hill, N.J., where he worked on developing high-precision communication-testing instruments; was formerly technical adviser to the Polish Stratospheric Board and associated with the National Institute of Telecommunication in Warsaw; 18 Apr.

Book Reviews

Bikini and Nearby Atolls. U.S. Geological Survey Professional Paper No. 260 (a-w). U.S. Geological Survey, Washington, D.C., 1954-1959 (order from Superintendent of Documents, Government Printing Office, Washington 25, D.C.). xv + 798 pp. Illus. \$24.15.

When the sea-worn H.M.S. *Beagle* returned to England in 1836 bearing the young naturalist Darwin and the collections he had accumulated, no one then living could have imagined the profound effect that such a modestly financed voyage would have upon the intellectual life of the modern world. Because in later years the *Origin of Species* received the lion's share of attention, and because it impinged upon such a diversity of interests, a second outgrowth of this unique voyage is often overlooked by the scientific world at large. This was Darwin's account of coral reefs and his theory that atolls had grown upward from subsiding foundations whose rate of sinking had been slower than the rate of upgrowth.

Strong support for Darwin's conclusions came from a contemporary explorer-naturalist, James Dwight Dana, who accompanied the United States Exploring Expedition on its voyage around the world under the command of Charles Wilkes. Two vessels of this expedition, the U.S.S. *Peacock* and the *Flying-fish*, visited Bikini, which was then uninhabited, in 1841.

In the following century, the subsidence theory was attacked repeatedly, and for a period, around World War II, there appeared to be almost as many theories proposed as there were investigators in the field. A leading thesis advanced in opposition to the idea of subsidence argued that, if the reef-bearing foundation remained stable, a platform upon which the reef might flourish could have been carved during a time when sea level was lower than it is today, perhaps during the glacial stages of the Pleistocene. This concept, with a variety of amendments added to it through years, was the chief con-

tender with the theory of subsidence. In addition to these two extremes, a number of intermediate hypotheses were advanced, embodying features of both.

Largely because the more imposing atolls are far from scientific centers and because they are such inscrutable structures, rising as they do from the abyssal depths of the open ocean, little more has been known of them than can be seen in the uppermost few fathoms; all else is shrouded in darkness. The obvious first step toward arriving at a solution to the problem of atoll formation would be to drill a hole through the coralline part of a reef and see how far down it actually extends. In part because of the distance and expense, only two holes had been drilled on reefs in the open Pacific before World War II—one to 1114 feet by the Royal Society, in 1896-98, at Funafuti atoll, the other by the Japanese to a depth of 1416 feet at Kaita-daito-jima, near Okinawa, in 1934-36. Although both holes revealed reef material all the way, the advocates of either theory were able to claim, by skillfully marshaling their arguments, that the imperfect evidence of these early bore holes supported their case.

Fortunately, the need for making a complete environmental study of the Marshall Islands atolls in connection with the earlier phases of the atom-bomb testing program in 1946 had as a scientific outgrowth the papers that have been gathered together to form this publication. The scientific community can be grateful indeed that such an opportunity was provided by the United States Navy in an enlightened approach to the values of pure science, very much in the tradition of the support given the pioneer investigations, in the last century, of Matthew Fontaine Maury in oceanography or of the Charles Wilkes expedition in marine geology, biology, geography, and ethnology. The comparison between the first naval scientific investigations of this distant part of the world and the investigation reviewed here is indeed

an interesting one. The intervening 105 years have seen an incredible advance in what might be described as the technology of science, yet one cannot fail to be impressed by the intellectual acuity of our predecessors.

The results of the many aspects of this detailed, postwar investigation of Bikini and the neighboring atolls in the Marshall Islands appear in a series of separate, paper-bound 9- by 11½-inch booklets, lettered consecutively from A to W, and together making up a single publication, U.S. Geological Survey Professional Paper No. 260. The 23 papers are attributed to 38 authors or groups of authors, but because some authors participated in the writing of more than one article, 29 people are actually involved. If all this sounds complicated, it is. It also makes it virtually impossible to review the work within the space available. The following list indicates the far-ranging nature of this inquiry:

Part 1, *Geology*. (a) "Geology of Bikini and nearby atolls," by K. O. Emery, J. I. Tracey, and H. S. Ladd, with foreword by Roger Revelle (1954).

Part 2, *Oceanography*. (b) "Circulation systems of Bikini and Rongelap lagoons," by W. S. von Arx (1954); (c) "Adjustment of Bikini atoll to ocean waves," by W. H. Munk and M. C. Sargent (1954); (d) "Sea temperature in the Marshall Islands area," by M. K. Robinson (1954); (e) "Biologic economy of coral reefs," by M. C. Sargent and T. S. Austin (1954); (f) "Plankton of northern Marshall Islands," by M. W. Johnson (1954); (g) "Recent brachiopods," by G. A. Cooper (1954); (h) "Recent foraminifera of the Marshall Islands," by J. A. Cushing, Ruth Todd, and R. J. Post (1954); (i) "Recent corals of the Marshall Islands," by J. W. Wells (1954).

Part 3, *Geophysics*. (j) "Seismic studies of Bikini atoll," by M. B. Dobrin and Beauregard Perkins, Jr. (1954); (k) "Seismic-refraction studies of Bikini and Kwajalein atolls and Sylvania guyot," by R. W. Raitt and Beauregard Perkins, Jr. (1954), (l) "Magnetic structure of Bikini atoll," by L. R. Alldredge, Fred Keller, Jr., and W. D. Dichtel (1954).

Part 4, *Paleontology*. (m) "Fossil calcareous algae from Bikini atoll," by H. W. Johnson (1954); (n) "Smaller foraminifera from Bikini drill holes," by Ruth Todd and Rita Post (1954); (o) "Larger foraminifera and smaller diagnostic foraminifera from Bikini

drill holes," by W. Storrs Cole (1954); (p) "Fossil corals from Bikini drill holes," by J. W. Wells (1954).

Part 5, *Miscellany*. (q) "Marine annelids from the northern Marshall Islands," by Olga Hartman (1954); (r) "Physical oceanography in the Marshall Islands area," by Han-Lee Mao and Kozo Yoshida (1955); (s) "Seismic reflection studies of Eniwetok atoll," by R. W. Raitt (1957); (t) "Chemical erosion of beach rock and exposed reef rock," by Roger Revelle and K. O. Emery (1957); (u) "Geothermal measurements on Eniwetok and Bikini atolls," by J. H. Swartz (1958); (v) "Larger foraminifera from Eniwetok atoll drill holes," by W. Storrs Cole (1957); (w) "Lower Eocene phosphatized globigerina ooze from Sylvania guyot," by E. L. Hamilton and R. W. Rex (1959).

The contributors have a wide variety of affiliations, other than the Geological Survey. This in itself is quite an innovation for publications by that organization. Among the institutions represented are the Naval Ordnance Laboratory, Pacific Ocean Fisheries Investigations, Scripps Institution of Oceanography, U.S. Navy Electronics Laboratory, U.S. National Museum, University of Southern California, and Woods Hole Oceanographic Institution. This departure from customary procedure introduces a variety of individualistic approaches into what is rather often a stylized format and is a welcome change indeed.

It is most difficult, from such a mass of observations and interpretations, to make a wise choice of the significant contributions without reflecting the prejudices or interests of the reviewer. To me, the principal value of this complex study is the detailed information it provides on the foundations of Bikini, presumably a representative coral atoll, and the ecology of the reef-building organisms and their associated marine flora and fauna.

Five drill holes were bored on Bikini in 1947, three shallow and two deep, the latter to depths of 1346 and 2556 feet, respectively. All the Bikini drill holes revealed calcareous, mostly unconsolidated or poorly cemented reef material all the way. Much of this material appears to have had a lagoonal origin and it seems to have been deposited at no great depth. This interpretation is corroborated by the contained fossils, which are shallow-water organisms whose ecological requirements

call for depths far less than those from which the fossils were recovered. The oldest rocks of the reef, to the depth penetrated by the drill, appear to be of Miocene age.

Both the lithologic and paleontologic evidence strongly suggests that Bikini atoll had an origin virtually identical with that proposed in Darwin's theory more than 100 years ago. That is, the reef grew continuously upward on a subsiding foundation whose rate of sinking was less than the rate of coral growth.

Three deep holes were drilled on nearby Eniwetok atoll in 1951 and 1952. The two deepest (4222 and 4630 feet) are exceptionally significant because they were drilled completely through the 4000-foot cap of shallow-water reef limestones and into the foundation rock, which proved to be an olivine basalt, much like that of other typical volcanic islands of the Pacific Ocean.

Although the holes drilled on Bikini are logged in great detail, no mention is made in the chapter on geology of the very significant Eniwetok borings, although the larger foraminifera recovered from their cores are described in part 5 (chapter v). One must look elsewhere for the description of the Eniwetok well logs. This appears to me to be a singular oversight (explainable, perhaps, by the long delay in publication). However, in view of the title of the professional paper, it would be most desirable to have all the data collected during the Marshall Islands investigations brought together in a single series—a procedure that was followed successfully by the great exploring expeditions of the 19th century, as in the *Challenger* reports.

The individual chapters (by several authors in most cases) are the heart of this publication and are essentially of specialist interest. The authors have done an admirable job of describing the physical environment and the marine inhabitants of Bikini and its neighboring atolls. This is probably the first time that such islands have had such intensive and quantitative investigation. The physical characteristics of the reefs, lagoons, and submarine slopes, ridges, and mountains are thoroughly mapped and described, together with the physical oceanography of the surrounding sea and the magnetic and seismic properties of the sea floor and the reef foundations. The biological sections of the report are equally im-

pressive and contain a fund of information on the distribution and ecology of both planktonic and reef-dwelling organisms. Algae are the dominant dwellers of the reef today, but corals are by no means out of the running. More coral species are known from Bikini atoll than from any other comparable area in the world, very likely as a result of intensive collecting during this expedition.

To one who knows little of this specialty, the fossil record appears to be unusually complete and exceptionally interesting. These deceased organisms not only provide us with an excellent insight into the environments of the past, but they also give us a means of dating the events in the geologic history of this remote archipelago. The coralline material at the bottom of the deep holes on Eniwetok, to judge from the foraminiferal content, was deposited in the Eocene. If this is the case, then the rates of subsidence called for are not excessive and are well within the limits of coral upgrowth. According to Cole (chapter v), the subsidence rate required for the 615 feet of Recent, Pleistocene, and Pliocene sediments would average 50 feet per million years; for the 2165 feet of Miocene deposits the rate would be 130 feet per million years; and for the 1773 feet of Eocene rocks the rate would be 170 feet per million years.

Phosphatized globigerina of Eocene age were recovered from deep-sea oozes that have sifted down into fractures in a volcanic tuff on the summit of Sylvania guyot, a flat-topped, plateau-like feature at a depth of 790 fathoms and a part of the same ridge on which Bikini atoll stands. This find, as described by Hamilton and Rex (chapter w), is of crucial importance in arriving at an understanding of these planed-off submarine mountains. The upper slopes of Sylvania guyot consist of basaltic rocks which are largely pyroclastic and which are believed to have been erupted when the water covering them was much shallower.

Many explanations have been offered for the mysterious, table-topped submarine mountains in the mid-Pacific, to which the name *guyot* has been applied in honor of Arnold Guyot, the Swiss geographer and associate of Louis Agassiz, and these are thought by some to be of very great geologic antiquity indeed. The evidence at Bikini of a mountain with an internal composition of largely unaltered basalt, capped with

globigerina ooze containing Eocene foraminifera, indicates a comparatively recent volcanic origin. Some time before the beginning of the Tertiary the volcanic mountain was truncated, and in the submergence that followed, the rate of sinking was too rapid for coral growth to keep pace. Therefore, no reef material accumulated as it did to a thickness of several thousand feet on the more slowly subsiding foundations of nearby Eniwetok and Bikini atolls.

There are a score of other contributions in this collection that are equally deserving of mention, but space is limited. Fortunately, the individual chapters can be obtained separately, and the title of each is sufficiently distinctive for a specialist to identify the ones most likely to be of interest to him. The range in price is wide, however, with a high of \$9.00 (for chapter a) and a low of 20 cents (for chapter s). The total cost (\$24.15) is likely to be too high for the average academician's already overextended book budget, or even for that of all but the most affluent institutional libraries.

The one element lacking in this stimulating collection of papers, I believe, is a section synthesizing the findings of the various contributors. This deficiency is remedied to some degree by Roger Revelle's foreword, but this is quite brief and was written largely before all the papers were published; it appeared in 1954, and chapters in this series continued to appear through 1959. The volume of material in this publication is too diverse and too highly specialized to be of general concern, yet the problem of coral reefs is of compelling interest, and a broad, comprehensive review of the findings of this 20th century expedition in terms of their relationship to past discoveries, to our present state of knowledge, and to the nature of problems still to be solved would have been most welcome.

The Geological Survey is to be congratulated on its excellent presentation of the results of this endeavor. The illustrations are of uniformly high quality, and the colored maps and charts of Bikini and other Marshall Islands atolls are superb examples of cartography; in fact, they are works of art in their own right.

The collating and publishing of this imposing accumulation of information of purely scientific interest is an achievement in the great tradition of the Survey's founders, notably such men as Powell, Gilbert, Dutton, and

Walcott. We can surely hope that an approach as successful as this one proved to be may establish a pattern to be employed more frequently in the future in a vigorous, wide-ranging series of investigations which may help us with the solution of the many riddles still confronting us about the nature of the earth and its past history.

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From Field to Factory. New industrial employees. James Sydney Slotkin. Free Press, Glencoe, Ill., 1960. 156 pp. \$4.

The central purpose of this study is to place the problem of labor in economic development (or, better, the problem of recruiting labor for industrialization) within a context of cultural anthropology. While the author is concerned with the problem of developing an anthropological theory about recruiting and committing labor in the underdeveloped areas now undergoing or about to undergo industrialization or modernization, he has not developed a series of interrelated propositions about this process.

What is of particular value in the study is the spelling out of a number of cultural factors that affect the industrialization process, but the propositions about the process are at such a high level of abstraction that they add little to the theoretical literature on industrialization or culture change written from other perspectives. In fact, the theory of culture change has advanced considerably beyond the framework of acculturation which is heavily used by Slotkin, and it is somewhat more sophisticated than is apparent from such conclusions as, "When a culture becomes inadequate in providing desired goods and services industrialism is adopted voluntarily" (page 143).

It has long been known that cultures are particularly prone to change under stress, but just what combination of conditions are most favorable for initiating the industrialization process in underdeveloped areas is not apparent from this study. As a first step to a general theory, however, the work is of value.

ALLAN R. HOLMBERG

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Science and State Government. A study of the scientific activities of state government agencies in six states. Frederic N. Cleaveland. University of North Carolina Press, Chapel Hill, 1959. xvii + 161 pp. Illus. \$3.50.

As an "interpretive summary" of the findings of reports on scientific activities in state government, this small volume tells the determined reader more than he wants to know and, possibly, more than he needs to know. It is long on fact, short on analysis and opinion; this is not always a virtue.

In 1954 the National Science Foundation contracted with the Institute for Research in Social Science at the University of North Carolina for a systematic study of science as a function of state government. Six states—California, Connecticut, New Mexico, New York, North Carolina, and Wisconsin—were selected as representative in their diversity. Using a common research design, teams of researchers converged on each of the states. The final reports afforded the basis for a statistical summary of the essential data by the National Science Foundation [*Scientific Activities in State Governments, Summary Report on a Survey Fiscal Year 1954* (Government Printing Office, Washington, D.C., 1958)].

Presumably this is the final effort to extract the last ounce of benefit from the reports, which must have been costly both in dollars and research travail.

If there is a "profile" of science in the six states, it is extremely fuzzy and indistinct. Indeed, the recitation of statistical differences tends to obscure the common features of scientific activity as a function of state government. There are common features—and significant ones—in the relationship of science to state government. But these are, perhaps, the ones least susceptible to analysis and appraisal by objective data.

Several observations are in order. First, the notion of shared responsibility between the federal government and the states in scientific activity is extravagant nonsense. The big money comes from Washington; the pattern and pace of government research effort is determined in Washington, whether in research on agriculture or on mental illness. Second, it is doubtful whether support for science in state government is "big business," as the author suggests. Less than 2 percent

of the state budget is expended in any of the six states for research; this is hardly impressive. Third, scientific activity in the states reflects the traditional obsessions, notably the heavy emphasis on agricultural research and on applied research generally. Perhaps the states may be "chasing the wrong rabbits"; research on urban redevelopment, housing, and smog may be more urgent than the search for new varieties of rust-resistant wheat. Fourth, the talents of researchers at the state university are rarely mobilized to bear on the crucial problems of a state. New Mexico, for example, needs a major, long-range research program on arid lands. The fact, cited in this study, that New Mexico puts the highest percentage of money into this kind of research obscures the central truth—namely, that the research effort is weak, thin, and uncoordinated, and falls far short of the need.

As a free people we have been content for the most part with a *laissez faire* philosophy regarding science. Our ideal is the researcher left to his own devices, pursuing his own interests. Perhaps it may not be inappropriate to suggest that scientific manpower resources can be mobilized in the cause of freedom without sacrificing the essential freedoms of the investigator. Whether at the federal or the state level, it is plain that this is the great challenge to science and public policy in our time.

HAROLD L. ENARSON

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Mathematical Methods and Theory in Games, Programming, and Economics. vol. 1, *Matrix Games, Programming, and Mathematical Economics.* x + 433 pp. vol. 2, *The Theory of Infinite Games.* xi + 386 pp. Samuel Karlin. Addison-Wesley, Reading, Mass., 1959. Illus. \$12.50 each.

Although there are several good books on game theory, none matches this set in completeness of exposition. These volumes present a thorough discussion of the essentially noncontroversial parts of the subject. Included in the first volume are a survey of discrete matrix games together with practical computational methods, the theory of linear programming, some results in nonlinear programming, and chapters

on applications to economics. The second volume is concerned with continuous games, including the several classes of readily solvable ones, and games of timing.

Karlin writes with a high degree of rigor that demands close attention from the reader; many fascinating problems are worked in detail. The price of these volumes can only be called breath-taking.

GEORGE WEISS

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University of Maryland

Plant Pathology. Problems and progress, 1908–1958. C. S. Holton, G. W. Fischer, R. W. Fulton, Helen Hart, S. E. A. McCallan, Eds. University of Wisconsin Press, Madison, 1959. xix + 558 pp. Illus. \$8.50.

This book contains 51 papers that were presented at the 50th anniversary meeting of the American Phytopathological Society. The papers, prepared by well qualified foreign and American scientists, embrace most of the broad field of plant pathology.

The first seven papers were major addresses devoted to the history and development of the science of plant pathology and to the history and development of the society.

The other 44 papers are arranged in nine groups corresponding to the symposia at which they were presented: (i) physiology of parasitism; (ii) genetic approach to elucidation of mechanisms governing pathogenicity and disease resistance; (iii) fungicides; (iv) chemistry of fungicides; (v) soil microbiology and root disease fungi; (vi) concepts and problems of nematology; (vii) structure of viruses; (viii) multiplication of plant viruses; and (ix) epidemiology of plant diseases.

These well prepared papers give a comprehensive summary of the present state of knowledge of the various segments of plant pathology. They are well documented, and each one includes an extensive list of pertinent literature citations.

This book gives a well-balanced review of the past and present state of plant pathology, and it will be a valuable reference book for teachers, research workers, and students.

HAROLD T. COOK

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Reprints

Adventuring with Beebe. Selections from the writings of William Beebe. Viking Press, New York, 1960. 282 pp. \$1.25. The selections, covering more than 40 years and ranging from Bermuda to British Guiana and the Pearl Islands, deal with varied wildlife from black inchling fish to 35-foot whale sharks.

Animal Camouflage. Adolf Portmann. Translated by A. J. Pomerans. Univ. of Michigan Press, Ann Arbor, 1960 (*Tarnung im Tierreich*, Springer, Berlin, 1956). 111 pp. \$1.95.

The Ants. Wilhelm Goetsch. Translated by Ralph Manheim. Univ. of Michigan Press, Ann Arbor, 1960 (*Die Staaten der Ameisen*, Springer, Berlin, ed. 2, 1953). 173 pp. \$1.95.

The Birds. Oskar Heinroth and Katharina Heinroth. Translated by Michael Cullen. Univ. of Michigan Press, Ann Arbor, 1960 (*Aus Dem Leben Der Vögel*, Springer, Berlin, ed. 2, 1955). 181 pp. \$1.95.

Caves of Adventure. Haroun Tazieff. Translated from the French by Alan Hodge. Viking Press, New York, 1960. 222 pp. \$1.45. An account (originally published in 1953) of an expedition into the labyrinth of caves 2000 feet underground in the Pyrenees.

The Chemical History of a Candle. A course of lectures delivered before a juvenile audience at the Royal Institution. Michael Faraday. William Crookes, Ed. Viking Press, New York, 1960. 122 pp. \$0.95.

Crucibles: The Story of Chemistry. From ancient alchemy to nuclear fission. Bernard Jaffe. Fawcett Publications, Greenwich, Conn., 1960. 240 pp. \$0.50.

Ebb and Flow. The tides of earth, air, and water. Albert Defant. Translated by A. J. Pomerans. Univ. of Michigan Press, Ann Arbor, 1960 (*Ebbe und Fult des Meeres der Atmosphäre und der Endfeste*, Springer, Berlin, 1953). 121 pp. \$1.95.

Engineers' Dreams. Willy Ley. Viking Press, New York, 1960. 240 pp. \$1.25.

The Foreseeable Future. Sir George Thomson. Viking Press, New York, 1960. 166 pp. \$0.95. This book, written in 1955, deals with the future of technology.

Light, Visible and Invisible. Eduard Ruechardt. 201 pp. Translated by Frank Gaynor. Univ. of Michigan Press, Ann Arbor, 1960 (*Sichtbares und Unsichtbares Licht*, Springer, Berlin, ed. 2, 1952). 201 pp. \$1.95.

Men of Medicine. Katherine B. Shippen. Viking Press, New York, 1960. 220 pp. \$1.25.

El Origen de las Especies por Medio de la Selección Natural. vols. 1 and 2. Carlos Darwin. Estudio preliminar de Juan Comas. Universidad Nacional Autónoma de México, México, 1959. vol. 1, 276 pp.; vol. 2, 296 pp.

Planet Earth. Karl Stumpff. Translated by Egon Larsen and Frank Pickering. Univ. of Michigan Press, Ann Arbor, 1960 (*Die Erde als Planet*, Springer, Berlin, 1955). 191 pp. \$1.95.

The Sun. Translated by A. J. Pomerans. Univ. of Michigan Press, Ann Arbor, 1960 (*Die Sonne*, Springer, Berlin, 1957). 160 pp. \$1.95.

Reports

The Need for Better Macromolecular Models

Abstract. Atomic models useful for small molecules become clumsy, expensive, and even inaccurate when used to represent the large molecules important in biology. More convenient models, authoritatively designed and semi-mass-produced, would be of the greatest value both for teaching and biological research.

Many of the most important current theories of biological structure and function at the molecular level depend on detailed space relationships between atoms in macromolecules. These include theories of DNA (deoxyribonucleic acid) structure, of protein synthesis and structure, of enzyme action, and of antigen-antibody relationships.

These theories are hard for anyone to understand in quantitative detail unless he has a three-dimensional mechanical model of the molecules to look at and manipulate. Unfortunately, the present commercial models—which are excellent for representing small molecules and have contributed tremendously to organic and inorganic chemical progress at the research level as well as at the teaching level—are unsatisfactory for these large biological molecules. As big-molecule models, they are expensive, clumsy, badly connected, and frequently fail to represent correctly, according to our best current knowledge, some of the essential structures, such as peptide bonds and aromatic rings. As a result, the advanced research laboratories in molecular biology commonly make their own macromolecular models in their own shops, often at a cost of thousands of

dollars for so elementary a model as a single turn of a DNA double helix. It follows that many laboratories and many good research minds outside these few centers are essentially excluded from participation in detailed discussion of such biochemical structural problems, and the new developments are more difficult to teach or to explain than they ought to be.

I believe that this lack of widely available macromolecular models is therefore a major bottleneck to our progress in biochemistry and molecular biology. It is the purpose of this note to suggest that it might be profitable if some national agencies or organizations interested in the biological field would set up an expert committee to look into the question of semi-mass-production of cheaper and more accurate macromolecular models, possibly under subsidy if necessary. The function of the committee would be to solicit ideas, to determine objectives and standards for such an operation, and to get it started. It would need to include workers in several fields, in particular, some x-ray or electron-diffraction experts on molecular dimensions; organic and quantum chemists, especially persons interested in steric effects; molecular biologists and biophysicists, who know what kind of accuracy is needed and what compromises could be made for mass production; persons who know the scientific apparatus market; and some who know the economics and technology of various methods of model manufacture, such as plastic molding. But the value to biological teaching and research might be many times greater than the cost of such a survey.

The possibilities that such a group could study may become clearer if I list several desirable features that I think could be incorporated in macromolecular models.

1) Molded plastic construction, possibly hollow, with ball-and-socket joints, like the "puppet head" necklaces that are often used now for lecture demonstrations of intertwined macromolecular helices, chromosome rearrangements, and so on. Models of this type could be light, well connected, and cheap, with costs measured in pennies per atom instead of dollars per atom as at present. The flexibility of the plastic

is not as great a drawback as some might think, because it can mirror the flexibility associated with the variable amplitude of thermal vibrations and with the "softness" of the van der Waals' radii in real molecules. An asymmetrical ball-and-socket joint can be used because biological chains commonly also have a built-in asymmetry or polarity of direction.

2) A small scale, possibly in the range from 2 to 5 mm/ \AA . Present models of large molecules are hard to hold together and to support because of their bulk and weight, which varies with the cube of the scale used and could be reduced by a factor of over 100 by going to a 2 mm/ \AA scale. Two different small scales might be useful, a very small one for examining long-range features such as secondary and tertiary coiling, and a larger one for greater accuracy in details.

3) Molding of rigid groups of atoms as single units, wherever possible. This would include aromatic rings, peptide bonds, cyclic sugars, phosphate groups, and $-\text{CH}_2-$, $-\text{NH}_2$, $-\text{OH}$, and $-\text{CH}-$ groups. This can increase by an order of magnitude the cheapness and accuracy of the models as well as the ease of assembly.

4) More accuracy in atomic shapes, for example in the thickness of aromatic rings (3.4 \AA), the planarity of conjugated amino groups (as in peptides), and the directional variation of non-bonding orbital radii in O, N, and S atoms.

5) Better methods for representing hydrogen bonds. Insert-magnets, with north pole for the donor, south pole for the acceptor, are said to have been tried and might be workable. A good representation of hydrogen bonds is important for many reasons, but one interesting one is that it would permit fitting water molecules accurately into and around the other molecular structures. This is essentially impossible in present models, although some have conjectured that it may be of the greatest structural importance in biology.

6) Representation of hindered rotation about bonds, for example by puckering the ball-and-socket connectors. A C—C single bond would have three favored angular orientations between the groups on the two ends. The physical barriers amount to 2 or 3 kcal/mole per bond and could add up to considerable energy in a long molecule with incorrect orientations. This means that the possible configurations of such molecules are much more limited than is generally appreciated from the use of present free-rotation models, and the choice of the best configuration might be biologically very important.

In the ideal model, some other lesser features may also deserve considera-

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should not repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to one 2-column figure (that is, a figure whose width equals two columns of text) or to one 2-column table or to two 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [Science 125, 16 (1957)].

tion. One would be the use of model materials of unit density. This would give the models neutral buoyancy in water and they could be suspended unsupported, so that their "spontaneous" three-space configurations would have greater fidelity. This could be of particular value in coiling-uncoupling and replication problems. Other useful inventions for an ideal model would be a way to represent strong ionic and electrostatic attractions, or the weak charge-transfer forces. In fact, it would be desirable, although it is probably not feasible, to represent all the major chemical force-fields with strengths more or less according to scale.

Some day it would also be most valuable to have molecular models that can represent a chemical reaction, by snapping from some ground-state configuration into some transition-state configuration and on over into the new configuration after the reaction. I tend to believe that certain DNA, enzyme, and other configuration problems may not be solvable by considering merely the static configurations in the dry state, and that we will have to think in terms of the dynamic transition-state configurations of the whole coiled system at the moment of attachment or reaction and in the presence of water.

However this may be, it would appear that it should be feasible to incorporate in molecular models at least some of the features I have mentioned. But I have listed them mainly to exemplify my main point, which is that molecular models could be made which would be cheaper, more widely available, more convenient, and yet more accurate in representing the essential features of macromolecular structures than any models we now have. The production of such models would be of the greatest importance to both research and teaching in the biophysical and biochemical sciences.

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7 March 1960

Blood Types in Fur Seals

Abstract. Individual variations exist in the erythrocyte antigens of fur seals (*Callorhinus ursinus*).

Studies of blood type are being conducted on several species of whales and fishes (1). They have the common aim of obtaining information useful in the characterization of intraspecific interbreeding populations similar to those that have been demonstrated in human beings and cattle (2). Our research shows that such studies have a poten-

Table 1. Summary of fur seal blood types so far differentiated by the absorption of rabbit anti-fur seal serum.

Type	Distinguishing antigens	Individuals collected and date (1958)
1	I, II, III	No. 636, 23 Mar.
2	I, III	No. 132, 24 Mar.; No. 637 and 639, 23 Mar.
3	III	All except those noted here
4	Negative	No. 329, 27 May

Table 2. Reactions of the serums of winter (1959) fur seals with fur seal erythrocytes.

Reaction	Asiatic coast	American coast
<i>A. Number of cell samples reacting positively with serum No. 408 (American coast)</i>		
Positive	2	0
Negative	142	27
Total	144	27
<i>B. Number of serum samples reacting positively with cells of individual No. 113 (Asiatic coast)</i>		
Positive	5	12
Negative	5	8
Total	10	20

tial for similar usefulness in the fur seal species (*Callorhinus ursinus*).

The fur seals of the North Pacific Ocean are noted for their interesting migratory and breeding habits. These seals winter in two separate groups off the northern coast of Asia and North America, respectively. These groups move north in the spring to reproduce on the Robben, Komandorski, and Pribilof islands. An international commission representing Canada, Japan, the United States, and the U.S.S.R. was established in 1957 to coordinate the management of this species (3). Present biological studies under this commission include efforts to determine the degree to which the total species population may be subdivided into separate breeding stocks, and the extent to which these stocks intermingle in winter. Starting in 1958 these efforts have included some exploratory work in blood typing and serology, carried on independently by Fujino in Japan (4) and by Ridgway in the United States (5). This paper reports on blood type variations discovered for the first time in this species by Fujino. These variations occurred among seals wintering off northern Honshu, Japan.

Table 1 summarizes the results of observations that demonstrated the individuality of these variations. At least four different blood types occur that distinguish certain individuals from the majority of seals sampled. While these individuals occur in low frequency, additional antigenic variations exist that

have not yet been sufficiently characterized. These include one within an antigen that resembles the species-specific human B-like antigen recently described in fin whales (6). Isoimmunization, although not yet attempted, would seem to offer a practical possibility for revealing further blood types in this species.

No natural antibodies have yet been found in serums of the horse, cow, pig, goat, and sheep that distinguish individual blood types. However, the normal serums of four rabbits all agglutinated type 1 cells (No. 636), and three of these also agglutinated type 2 cells (No. 639). Titers averaged one in eight.

Isoagglutinins also occur at high frequency. A series of 12 serums contained seven that agglutinated type 1 cells (No. 636), plus one that agglutinated both type 1 cells (No. 636) and type 2 cells (No. 639). Titers averaged one in two. Table 2 outlines a simple method of obtaining data on the frequency of isoagglutinin positive types. (In developing this method recognition must be given to the fact that the isoantibodies in some serums are lost after several freezings and thawings, and that therefore isosera should be divided into aliquots while they are still fresh.)

While the 234 samples studied in 1958 were collected rather evenly over a range of 4 months (March through June), all individuals of types 1 and 2 were taken on 23 and 24 March. Similarly, out of 144 samples tested in 1959, both isoagglutinin positive individuals were collected on 2 April. These samples were collected during March, April, and May. This nonrandom distribution of types in the early part of the season is suggestive that some localization of breeding stocks is maintained within the winter population from year to year.

The glycerol-freezing technique, as applied in the blood typing of whales (7), has been successfully used to preserve intact erythrocytes of fur seals for several months. This observation, together with those on antigenic variations summarized above, leads to the conclusion that there is a good probability that blood type antigens could be used to advantage in research on fur seal populations (8).

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This work was supported by grants from the Japanese Fisheries Agency; the John Simon Guggenheim Memorial Foundation (J.E.C. was a fellow in 1958-59); and the biology branch of the U.S. Office of Naval Research. We acknowledge with thanks the arrangements made by Dr. H. Omura (director of the Whales Research Institute, Tokyo), Prof. Y. Suyehiro (chief of the department of fisheries, Tokyo University), Mr. F. Nagasaki (chief of marine mammals section, Tokai Regional Fisheries Research Laboratory, Tokyo), Dr. G. J. Ridgway (U.S. Fish and Wildlife Service) which made this cooperative research possible. Serums and cells from American seals were provided by Dr. F. Wilke (U.S. Fish and Wildlife Service). The technical assistance of Mr. K. Takahashi (Tokyo University) is gratefully acknowledged.

14 December 1959

Gene Flow and Divergence under Disruptive Selection

Abstract. Two halves of a population exposed to selection in opposing directions can diverge despite gene flow of the same amount as is given by random mating. Divergence was as great as it is with complete isolation. Isolation, therefore, is not a prerequisite of divergence under divergent selection pressures.

Thoday and Boam (1, 2) have demonstrated that two halves of a population of *Drosophila melanogaster* can diverge when selected for opposite extreme values of a metric character (sternopleural chaeta number) even though all the individuals of every generation are the progeny of hybridization between the two halves. They have pointed out that their results throw doubt on the assumption that isolation is a necessary prerequisite of such divergence in natural populations.

The population maintained by Thoday and Boam involved forced gene flow such that the two halves of the population exchanged 50 percent of their genes in each generation. This is twice as much gene flow as the maximum usually considered in relation to natural populations, for random mating involves only 25 percent gene flow. We have accordingly run two selection lines under disruptive selection with positive assortative mating, using a

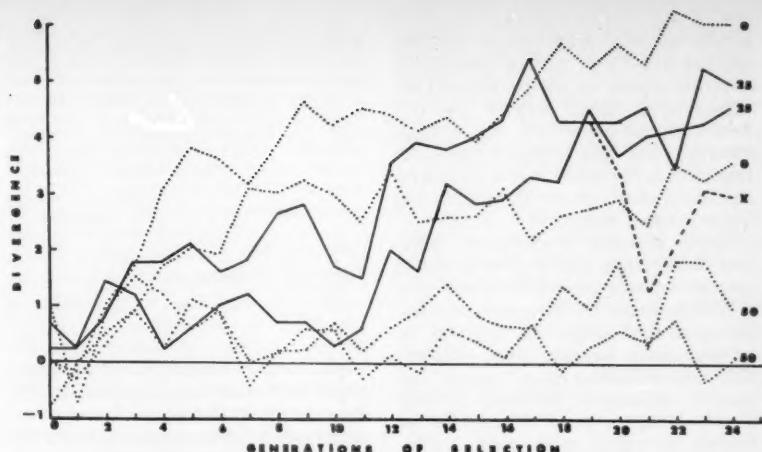


Fig. 1. Differences, in chaetae per fly, between the mean for the high half and that for the low half of each line in each generation. Dotted curves, the 0 and 50 percent gene-flow lines. Solid curves, the 25 percent gene-flow lines. Broken curve (X), the 25 percent gene-flow negative-assortative-mating subline.

system that gives 25 percent gene flow by male migration (Table 1). For comparative purposes, two lines with 50 percent gene flow (which differed from that of Thoday and Boam in that males were used for migration), and two with no gene flow (complete isolation) were maintained. All originated from the wild stock that was used by Thoday and Boam (1-3). Culture conditions and the proportion of flies selected were also the same.

The results are presented in Fig. 1, in which the differences between the mean chaeta number of the high and that of the low halves of each line are plotted for each generation.

Fifty percent gene flow permitted some divergence, though it is clearly a great restriction. The divergences of the 25 percent gene flow populations are very much greater. Though slower to develop than those permitted by complete isolation (no gene flow), their magnitude is of the same order. This might not be so if the population sizes were larger, but it provides a striking

demonstration that very considerable divergence is possible without isolation. The mean chaeta numbers of these populations are of the order of 19, and the differences between the high and low halves of the 25 percent gene flow populations must be considered very large in relation to this mean.

The relevance of these results to theories related to natural populations is somewhat limited by the fact that the flies which convey genes from one half of a population to the other are selected for chaeta numbers deviating in the direction appropriate to the half population to which they are made to migrate. We have therefore taken a subsidiary line from one of the 25 percent gene flow populations and maintained it under 25 percent gene flow, disruptive selection with negative assortative mating. That is to say, the flies that are to carry genes from the low-to-the-high-chaeta-number half of the population are selected for low chaeta number, and those that migrate from high-to-low halves are selected for high

Table 1. Mating and selection system used for disruptive selection with 25 percent gene flow. The entries designate the flies chosen to perpetuate each of four female lines in each generation. H indicates the highest, and L the lowest chaeta number fly found in the appropriate culture. The letters A, B, C, and D indicate the culture from which the fly was selected.

Female line	Female parent	Male parent			
		1	2	3	4
High half population					
A	AH	BH	DH	BH	DH
B	BH	CH	AH	CH	AH
Low half population					
C	CL	DL	BL	DL	BL
D	DL	AL	CL	AL	CL

chaeta number. The results are also illustrated in Fig. 1. The change of selection system, of course, reduced the difference of mean between the two halves of the population. But a large difference has been retained. There can therefore be no doubt that a considerable difference could be maintained under random mating.

These selection experiments therefore demonstrate that it is in principle possible for ecotypes or biological races to diverge under the divergent selection pressures that might be imposed in heterogeneous habitats. The concept that random mating must "swamp" the genetic differences involved is not sound. Isolation barriers may be involved in much, even most, evolutionary divergence, but they are not a prerequisite of such divergence (4).

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16 July 1959

Enzymatic O-Methylation of N-Acetylserotonin to Melatonin

Abstract. An enzyme, hydroxyindole-O-methyl transferase, that can transfer the methyl group of S-adenosylmethionine to the hydroxy group of N-acetylserotonin to form the hormone melatonin is described. This enzyme, which is highly localized in the pineal gland, also O-methylates serotonin.

Recently Lerner and co-workers (1) isolated a new hormone, melatonin (N-acetyl-5-methoxytryptamine), from the pineal gland and peripheral nerves of man, monkey, and cow. This compound was found to lighten the color of frog melanocytes and block the actions of the melanocyte-stimulating and adrenocorticotropic hormones (1). McIsaac and Page have recently shown that

Table 1. Enzymatic O-methylation of N-acetylserotonin to melatonin. The soluble supernatant fraction obtained from 16 mg of cow pineal gland was incubated at 37°C with 0.1 μmole of N-acetylserotonin, 100 μmoles of phosphate buffer (pH 8.0), and 0.1 μmole of S-adenosylmethionine. After 2 hours' incubation melatonin was determined in the incubation mixture (4).

System	Melatonin formed (μmoles)
Complete system	11
S-adenosylmethionine omitted	0

serotonin (5-hydroxytryptamine) is converted to N-acetylserotonin in vivo (2). We wish to report the isolation of an enzyme that forms melatonin by the O-methylation of N-acetylserotonin.

Since melatonin was found to be highly localized in the pineal gland (1), this tissue was examined for the presence of an enzyme that could O-methylate hydroxyindoles. Pineal glands from cows (3) were homogenized with ice-cold isotonic potassium chloride and centrifuged at 78,000g. The resulting soluble supernatant fraction was incubated with N-acetylserotonin and S-adenosylmethionine at pH 8.0. After a 2-hour incubation at 37°C the reaction product was extracted from the incubation mixture with chloroform and the organic phase was washed with water to remove residual substrate. The chloroform extract was then evaporated to dryness in a stream of warm air and the residue taken up in 3N HCl. A fluorescent metabolite was found to be present in the acid extract with a maximum fluorescent peak in 3N HCl at 540 mμ upon activation at 310 mμ; this is characteristic of 5-hydroxy- and 5-methoxyindoles. This metabolite had the same fluorescent spectrum, the same R_f values in butanol, acetic acid, and water (100 : 35 : 70) (0.91) and in N-propanol and 1N ammonia (5 : 1) (0.89), and the same color reactions and partition coefficient as authentic melatonin. When S-adenosylmethionine was omitted from the incubation mixture, no melatonin was formed (Table 1). These observations demonstrate the existence of an enzyme (hydroxyindole-O-methyl transferase) that can transfer the methyl group of S-adenosylmethio-

nine to the hydroxy group of N-acetylserotonin. The reaction is shown in Fig. 1.

Hydroxyindole-O-methyl transferase has been purified about 20-fold from beef pineal gland by heat treatment, ammonium sulfate fractionation, and adsorption and elution from alumina C_γ gel (4). Unlike catechol-O-methyl transferase (5), the enzyme has no requirement for Mg⁺⁺. It could not be detected in liver and kidney of a number of mammalian species, but was found in the pineal gland of the monkey (4). The lack of the requirement for Mg⁺⁺ and the unique localization of hydroxyindole-O-methyl transferase indicates that it is different from catechol-O-methyl transferase (5) and the other known transferases (6, 7).

Incubation of serotonin with hydroxyindole-O-methyl transferase and S-adenosylmethionine resulted in the formation of a product having the characteristics of authentic 5-methoxyserotonin. However, the rate of O-methylation of serotonin was only one-tenth that of N-acetylserotonin (4). This finding suggests that acetylation precedes O-methylation in the formation of melatonin as follows:



From the results described in this report and elsewhere, it is becoming increasingly apparent that O- and N-methyltransferases (5-7) requiring S-adenosylmethionine are playing key roles in the biosynthesis and inactivation of biologically active amines and their derivatives (8).

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21 December 1959

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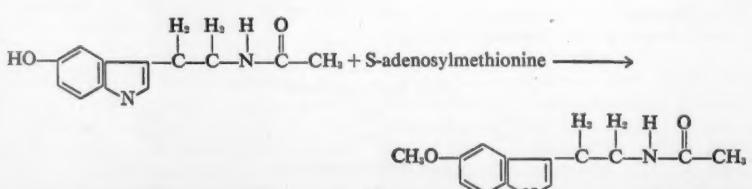


Fig. 1. Transfer of the methyl group of S-adenosylmethionine to the hydroxy group of N-acetylserotonin.

Amber with Insect and Plant Inclusions from the Dominican Republic

Abstract. An amber-bearing formation observed in the Dominican Republic in 1959 is described, and several insect orders, spiders, and plants found in amber are noted. An amber trace is recorded for Haiti, and the known fossil-bearing amber deposits or sites in the New World are summarized.

Amber from what is now the Dominican Republic was first reported by Christopher Columbus between September 1494 and March 1496, during his second voyage to the West Indies. The original record of this discovery has not been verified, but according to Hale (1), who examined Columbus' documents, amber was discovered in a mining region near the Tower of Conception—a fortress built on the border of the country ruled by the cacique, Guarionexius.

During the next 400 years, no further references to Dominican amber appear to have been recorded. In 1905 (2) an amber-bearing formation in the Monte Cristi Range (Cordillera Septentrional) was described, and the author added that the amber "... frequently contains inclusions of dirt and decayed twigs." Other reports followed, which discussed the geology and uses for amber (3, 4, 5, 6), and in one of these reports (4) the author stated, "Inclosed in this resin are oftentimes small fragments of lignites, leaflets [sic] or insects such as mosquitoes, ants, etc."

The amber-bearing formations in the Dominican Republic are located at two principal sites in the Cordillera Septentrional north of Santiago between Altamira and Canca (4), at an approximate elevation of 1240 m. The original site is the Peña (Tamboril) region in the two gorges (Los Meninos and Perez) of the Arroyo Capancho tributary of the Rio Gurabo. The second site is below Pico Diego de Ocampo near Pedro Garcia in the Palo Alto de la Cumbre region, and the length of the vertical outcrop here is approximately 300 m (7).

During May 1959 (8) we visited the second site and collected samples of amber and associated sandstones. Samples and photographs of the section examined (9) indicate an uppermost leached layer varying in thickness up to 15 m (6). Below this layer in this section was a soft layer of clay shale varying from $\frac{1}{2}$ to 2 m in thickness, followed by a harder layer of silty shale 2 to 2.5 m thick. Below the latter was a fourth layer of unknown thickness, a grey sandstone in which the amber occurred. Contacts between these units are gradational. The sandstone varies in

color from light brown to dark grey, and it is a fine-grained, micaceous and carbonaceous, and laminated graywacke. The amber, which is confined to a thin bed, is removed by breaking chunks of sandstone, first removed with a pick, by hand or with a heavy knife. The porous chunks may be softened after a few days' exposure to the atmosphere.

The amber occurred irregularly as small broken fragments to large pieces in original unbroken form. The three largest pieces examined, of which the two smaller had been stream tumbled, weighed 89, 140, and 245 gm. Transparent, unfractured pieces were common, but many small broken pieces were brittle and contained numerous fractures. The color varies from crystal clear through various shades of honey to deep red.

Inclusions occurred in about one of every 15 or 20 pieces. Noted in our collections, also in the collection of Brouwer (7) were several insect orders, including Blattaria, Isoptera, Corrodentia, Heteroptera, Hymenoptera, Homoptera, Coleoptera, Lepidoptera, and Diptera. Also observed were spiders, fragments of wood, roots, flowers, leaves, and air bubbles.

The Dominican amber is believed by Brouwer to be Oligocene in age (6), but exact dating remains in doubt (10) pending current investigations. Similarly, the source of this amber is not definitely known, although it possesses a strong pine-like odor when sawed or scratched with a file or sandpaper.

Small deposits or traces of amber have been reported for many parts of the world. An amber trace, identified by chemical analysis, was noted in a core sample from a lignite deposit near Maissade in the Plateau Central of Haiti (11).

The amber deposits of the world known to bear insect and other arthropod remains are few. In addition to the celebrated Prussian Baltic amber deposits of Eocene, or pre-Eocene (12), and Lower Oligocene age (13, 14), others are known to occur in Sicily, Romania, Burma, Zanzibar, Indonesia (15), and the Philippine Islands (15). In North America the first known amber insect, a caddis fly, was described in 1917 from Cretaceous amber from Coffee Bluff, Hardin County, Tennessee (16). However, there is an earlier reference in 1879 (17) mentioning "ants, a fly, and probably small species of Coleoptera" in an amber sample taken on Nantucket Island, Massachusetts. Canadian amber of Cretaceous age from Cedar Lake, Manitoba, with insect inclusions was mentioned first by Walker (18) and later discussed in detail by Carpenter et al. (19). Buddhue (20)

recorded a fossil gum from Baja California, in one piece of which a whitish bee-like insect and a grub were included. Recently, various authors have reported or discussed two additional sources for North America, one of Late Early or Early Late Cretaceous age from the slopes of the Arctic Ocean near Wainwright, Alaska (21), the other (22) of Oligocene and Miocene age from the vicinity of Simojovel in Chiapas, Mexico (14, 23).

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8. Edward L. Mockford informed us in 1956 that he had examined several pieces of Dominican amber during a visit in 1954.
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17 February 1960

Opponent Color Responses in Retinal Ganglion Cells

Abstract. The receptive fields of certain ganglion cells in the goldfish retina have been mapped. These fields are "off" center, "on-off" intermediate, and "on" periphery types. The excitatory process controlling the "on" response is stimulated maximally by green light; the "off" response process, inhibitory in nature, is stimulated maximally by red light. The two processes can be light adapted independently.

We have recorded the electrophysiological responses to monochromatic illumination of one type of ganglion cell in the retina of the goldfish. These cells give "on-off" type responses similar to those found in other vertebrate retinas when illuminated (1). Figure 1 is a plot of the intensities necessary to evoke a threshold response at "on" and at "off" for various wavelengths of the stimulating light. For the particular cell shown, "on" thresholds could be obtained from about 425 to about 600 m μ but not at longer wavelengths. "Off" thresholds were obtained from about 530 to about 750 m μ . These thresholds seem to define two separate but overlapping spectral luminosity functions, one associated with the "on" response, the other with the "off" discharge. In the region of overlap of the two functions an increase in the stimulus intensity above that of the "off" threshold diminishes or abolishes the "on" response. Reillumination at any wavelength above the "off" threshold tends to inhibit any "off" discharge following a previous stimulation.

The "on" and "off" processes could be light adapted separately. Adaptation to a steady background of red light

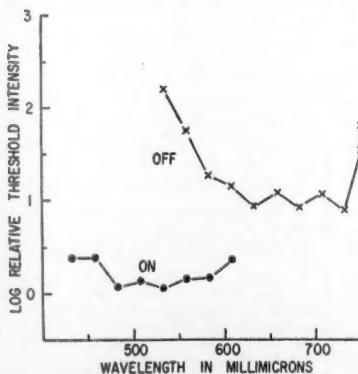


Fig. 1. Intensity necessary to elicit various types of threshold responses from a single ganglion cell at different wavelengths. Each point is an average of four determinations at that wavelength. The duration of the stimulus was 1.0 sec; 0 log units = 5.5×10^{-2} μ watt/cm 2 for all wavelengths. Stimulus diameter, 5 mm.

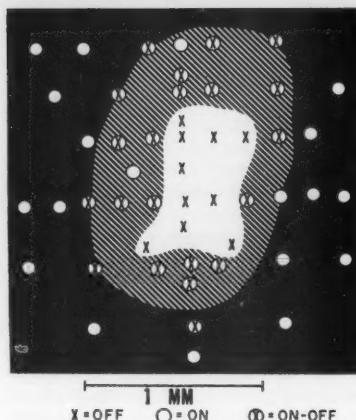


Fig. 2. Receptive field of a single ganglion cell. Central clear area indicates region where only "off" responses were found. Hatched area indicates region where "on-off" type responses were found. "On" responses were found only in peripheral area indicated by solid black. Test stimulus was 153 μ in diameter, wavelength 600 m μ , intensity 18 μ watt/cm 2 .

raised the threshold of the "off" response, but the threshold of the "on" response was slightly lowered. A blue adapting light made the "on" process less sensitive and the "off" process more sensitive.

Most of the ganglion cells studied had their "on" maxima at about 525 m μ and their "off" maxima at about 620 m μ , which suggests that this kind of cell may be connected to both porphyropsin and cyanopsin systems (2).

Figure 2 is a plot of the response patterns observed when the receptive field was explored with a small spot of 550 m μ wavelength. The "off" center, "on" periphery character is evident.

The two antagonistic processes may indicate the presence of a Hering type opponent color mechanism in the goldfish retina which is encoded not only in terms of wavelength but also spatially in terms of the receptive fields of the ganglion cells (3).

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References and Notes

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3. The opinions or assertions contained herein are ours and are not to be construed as official or reflecting the views of the Navy Department or the naval service at large. This research was supported in part by National Science Foundation grants G-3321 and G-7086.

8 December 1959

Chlorophyll-Sensitized Photoreduction in the Thionine-Ferrous System

Abstract. The reduction of thionine in aqueous solution to leucothionine by ferrous ions in light can be sensitized by chlorophyll in the colloidal state, as obtained by diluting alcoholic solution with water.

The ability of chlorophyll in the molecularly dispersed state to catalyze various oxidation-reduction reactions is well known (1-3). Recently some evidence was found of a similar ability of chlorophyll in colloidal solutions, prepared by dilution with water of solutions of chlorophyll in alcohol, pyridine, or acetone (4).

We have studied the photochemical behavior of the thionine-ferrous iron system in aqueous solution (see 2, 5) and have found that, in the presence of colloidally dispersed chlorophyll, reversible photoreduction of thionine can be produced in red light absorbed only by chlorophyll ($\lambda > 650$ m μ).

The ferrous sulfate (analytical reagent grade) was used without further purification; thionine (biological stain of the National Aniline and Chemical Co., Inc.) was purified by repeated recrystallization. Chlorophyll (a or b) was prepared from fresh spinach by the method of Zscheile and Comar (6). The purity of the chlorophyll and of the thionine was checked by chromatography and spectroscopy.

A fresh aqueous solution of thionine ($\sim 5 \times 10^{-3} M$), containing ferrous iron ($\sim 10^{-3} M$), was placed in the main, square optical cell (1 cm 3), while a fresh ethanolic solution of chlorophyll was placed in a side tube, connected to the main cell by a closed capillary. To remove oxygen, both solutions were pumped out on a high-vacuum line; the residual oxygen pressure was $< 10^{-6}$ mm-Hg.

The thionine-Fe²⁺ cell, immersed in a thermostat, was first illuminated in the absence of chlorophyll, by a 1000-watt incandescent lamp, through a glass filter cutting off infrared radiation, and a red interference filter ($\lambda_{max} = 645$ Å). A battery-operated ribbon filament lamp (6 volts, 18 amp), focused first on the sample and then on the entrance slit of a Farrand monochromator, provided the scanning beam. Its intensity was reduced by a variable

aperture until it produced no measurable photolysis. The transmittance of the solution at 600 m μ (thionine band) and 680 m μ (chlorophyll band) was recorded by means of a photomultiplier, using a Tektronix 502 oscilloscope. As expected, in the absence of chlorophyll, the optical density at 600 m μ was not affected by illumination with red light.

The capillary in the side tube was

then broken and alcoholic chlorophyll solution was transferred into the aqueous solution in the main cell, giving a colloidal chlorophyll solution with a concentration of $\sim 10^{-5} M$. The color of the mixture was bluish green. Its absorption spectrum, determined with a Beckman DU spectrophotometer, showed a rather flat peak at 670–690 m μ characteristic of chloro-

phyll in the colloidal state, and the normal sharp absorption peak of thionine at 600 m μ (Fig. 1). The mixture did not undergo decoloration after standing for 2 to 3 weeks in the dark under anaerobic conditions, which indicates that colloidal chlorophyll did not react with thionine or ferrous iron in the dark. After the mixing, the cell was immersed in a thermostat, kept for 5 minutes for temperature equilibration, and then illuminated with red light. No measurable change of optical density at 680 m μ , due to chlorophyll, could be observed in light; but the optical density at 600 m μ , due to thionine, decreased as plotted in Fig. 2.

Similarly to the direct photochemical reduction of thionine by ferrous ions, the photosensitized oxidation-reduction of thionine was completely reversible; after several cycles, the absorption spectrum of the suspension was the same as before the illumination.

Sensitization could involve either reversible photoreduction of chlorophyll by ferrous ions, as in Krasnovsky's interpretation of chlorophyll-sensitized reduction of safranine by ascorbate, or it may be based on energy transfer from chlorophyll to thionine. Such a transfer is impossible in the singlet state, because of the relative position of the excited levels; but if thionine, similarly to the related compound acridine (7), has a particularly low-lying triplet state, energy transfer may conceivably occur via the triplet state of chlorophyll.

The capacity of colloidal chlorophyll to photosensitize endergonic oxidation-reduction could be of some interest in relation to its role in photosynthesis.

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7 March 1960

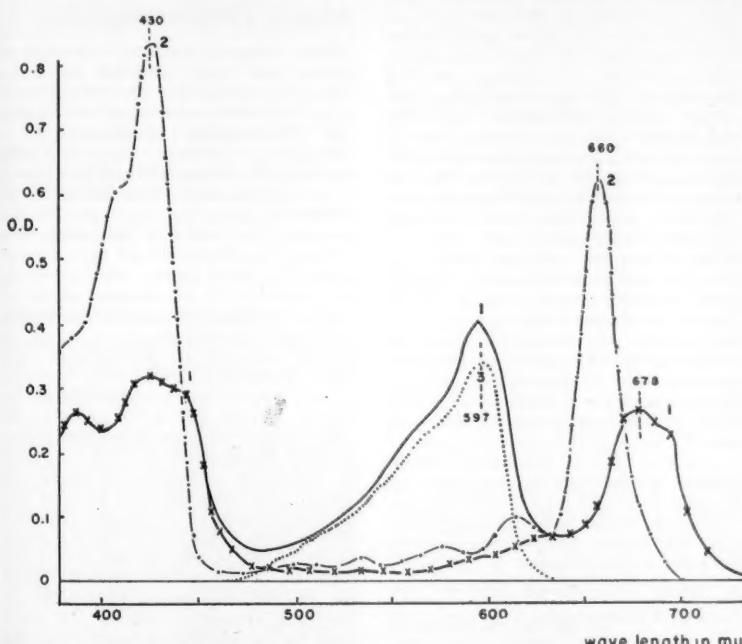


Fig. 1. Absorption spectrum of thionine solution in 30-percent aqueous methanol containing colloidal chlorophyll, compared to the absorption spectra of thionine in alcohol and of chlorophyll solution in ether. [Absorption curve of thionine in 30-percent aqueous alcohol is very similar to that in pure alcohol (see 8).] 1, Suspension containing chlorophyll *a* and thionine; 2, chlorophyll *a* in ether 10^{-5} mole/liter; 3 thionine in alcohol 5×10^{-6} mole/liter. Crosses indicate the absorption curve of colloidal chlorophyll solution without thionine.

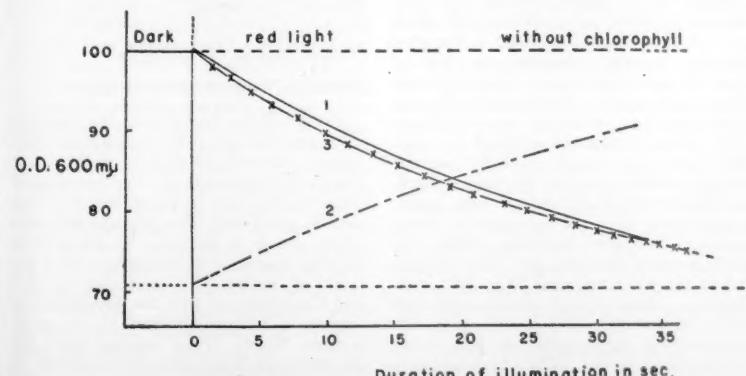


Fig. 2. Direct and colloidal chlorophyll sensitized reversible photoreduction of thionine by ferrous iron. Concentration of chlorophyll, 10^{-5} mole/liter; thionine, 5×10^{-6} mole/liter; ferrous iron, 10^{-5} mole/liter; at 20°C . 1, Photoreduction of thionine by red light sensitized by chlorophyll; 2, back reaction; 3, photoreduction of thionine by white light (without chlorophyll).

National Academy of Sciences

Abstracts of Papers Presented at the Annual Meeting, 25–27 April 1960, Washington, D.C.

Forecast of United States Precipitation through 1967

A family of regular harmonic periods of identical lengths in the sun's radiation and in weather has been discovered. The periods are exact submultiples of 273 months. In weather they have hitherto been unrecognized. Though the periods remain unaltered, their phases alter with seasons of the year, with sunspot frequency, and also secularly. Having eliminated these phase changes, I have determined average forms and amplitudes of 27 periods from monthly records since 1870. By adding the thus-cleared monthly-expressed 27 harmonic periods, monthly departures from the normal precipitation for the years 1950 through 1967 are predicted for 32 cities. Correlation coefficients of 50 to 70 percent are found for the 9-year interval 1950–58. Similar correlation is to be expected 1959–67.

This work is sponsored by the Association for Applied Solar Energy and by the Smithsonian Institution.

C. G. ABBOT

Smithsonian Institution

Relation of Photosynthetic Transients to Respiration

It was suggested last year that the time course of "chromatic transients" in photosynthesis could be ascribed to characteristic changes in respiration during the first few minutes of exposure to light of different wavelength. This hypothesis, based on *Ulva*, has now been verified in a more favorable green alga, *Enteromorpha*, a tubular form in which the tissue is but one cell thick, and in which the plastids are sometimes aligned along the outer, very thin cell wall. When this tissue is held tightly against a polarized platinum electrode, the latter shows especially rapid responses to oxygen released in photosynthesis, or respired in the dark. It is possible to follow the respiration at very short intervals after varying durations of illumination, and at critical points along the photosynthetic transient.

During exposure to light of 650 m μ (absorbed by chlorophyll *b*) or 490 m μ (absorbed by carotenoids) there is little change of respiration during the initial cusp; a striking increase during the depression; and a gradual decrease toward normal in the recovery phase. After long exposure to light there is sometimes a delayed increase of respiration, appearing

a minute or two after darkening. Conversely, during illumination with long wavelengths (702 m μ , absorbed only by chlorophyll *a*), the respiration is at first enhanced, but falls off during the next minute or two. This accounts for the slow rise of O₂ evolution in the far red, and the absence of an initial cusp.

These respiratory changes seem adequate to explain the chromatic transients found on going directly from far red to shorter wavelengths and vice versa. In one specimen of *Enteromorpha* (kept for some time in dim light) the chromatic transients and respiratory changes remained large even through the "Emerson effect" (enhancement) was essentially absent.

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Submicroscopic Changes in "Paling" of Irradiated Parts of Chromosomes

A small part of a chromosome, after appropriate exposure to ultraviolet light, shows a decrease in refractive index ("paling"), a decreased absorption of light at 260 m μ , and greatly diminished or negative reactions with basophil dyes and the Feulgen and pyronin-methyl green stains. These findings strongly suggest that in the irradiated area most, if not all, of the deoxyribonucleic acid (DNA) is either lost or greatly changed. In electron micrographs of unstained cells fixed in 10 percent neutral formalin in Tyrode's solution, the chromosomes are seen to have sharp outlines and a homogeneous appearance except in the irradiated areas where they are markedly vacuolated and have less circumscribed margins. In similarly fixed cells stained with 0.5 percent phosphotungstic acid in absolute alcohol, the chromosomes consist of two major components: (i) a homogeneous or finely granular, pale grey substance which fills the interstices between and (ii) delicate rod or tubelike structures 75 to 300 Å in diameter. These branch occasionally and extend several thousand angstroms in the thin sections. Some continue into interchromosomal areas. The homogeneous substance diminishes progressively from nonirradiated portions of chromosomes through a transitional zone into the heavily irradiated region where it is apparently absent, leaving only a network of tubules.

It thus seems that the grey substance,

which disappears after irradiation, contains DNA alone or associated with protein. (Osmic acid solutions, the usual fixatives for electron microscopy, do not show differences in structure between the irradiated and nonirradiated portions of the chromosomes, presumably because they do not demonstrate DNA.)

WILLIAM BLOOM

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Energy Relations in Chromosome Paling by Ultraviolet Microbeams

The characteristics of chromosome paling have been described by Bloom (foregoing abstract). This phenomenon offers opportunity for study of chemical and ultrastructural disassembly of a chromosome within a living cell. One approach to understanding of its nature is to obtain quantitative data concerning the ultraviolet energy relations, absolute and spectral. The following information was obtained in experiments on newt chromosomes in tissue culture with ultraviolet microbeams 8 μ in diameter. Based on number of incident quanta required to produce a standard degree of paling, the relative actions of wavelengths 2250, 2400, 2500, 2600, 2700, 2800, and 3000 Å are proportional to 220, 30, 10, 16, 19, 16 and 2.2. This action spectrum deviates widely from the absorption spectra of deoxyribonucleic acid (DNA) and of its constituent nucleotides but agrees reasonably well with those of proteins containing tyrosine or tryptophan, or both. With a 2600-Å microbeam, paling is produced in 50 percent of the cells by an exposure of 1.2×10^6 quanta/μ². From this number and additional data in the literature, a minimal value of 2 is calculated for the number of quanta absorbed per DNA nucleotide initially present. However, from detailed consideration not only of the foregoing data but of certain qualitative aspects of paling, it is concluded that the photons involved in paling are not those absorbed by the DNA but more probably start their action by absorption in a protein.

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Vector Meson Coupling in Nucleon-Nucleon Interactions

Evidence for considering the repulsive core and the spin-orbit interaction which appear indicated by nucleon-nucleon scattering as originating in the coupling of nucleons through a heavy vector meson field is presented. The approximate mass of the meson is estimated as about 10 or 12 pion masses. It is shown that the treatment of antinucleon scattering by Ball and Chew falls in with the hypothesis requiring a change believed to be unimportant. Heavy vector mesons have apparently not been directly observed in antinucleon-nucleon collisions or other circumstances. This situation is reviewed. It is shown that for the most likely process the energy used in experiments was too low. Difficulties which the hypothesis might encounter with the binding

energy of H^0 are discussed with the conclusion that there are many possibilities for reconciliation. Partial support for the hypothesis is found in the comparison of data on the photodisintegration of the deuteron with calculations employing fixed potentials and neglecting exchange currents. Similar qualitative support is found in the simplicity of electromagnetic form factors derived from e-p and e-d scattering experiments, the direct effect of the vector meson coupling being to smear the proton and neutron charge and current distributions equally. This corresponds to the naive interpretation of empirical evidence. The view proposed leads to more nearly energy independent depth and range parameters of nucleon-nucleon potentials than would be expected on the pion theory without the vector field.

This research was supported by the U.S. Atomic Energy Commission under contract AT (30-1)-1807 and by the Office of Ordnance Research, U.S. Army.

G. BREIT

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Effects of Structural Changes in the Long Arm of Chromosome 10 in Maize on Paramutation of the R' Factor

Three reciprocal translocations involving, in each case, a break in the long arm of chromosome 10, two proximal and one distal to the R locus and removed from the latter by at least 5 to 10 crossover units, if in coupling but not repulsion with R' , (i) increase the aleurone pigment-producing action of R' from dark mottling to near self-color, in single dose, and (ii) render R' comparatively insensitive to paramutation in stippled (TR'^+) heterozygotes (TR'/R'^+ or $R'T/R'^+$). Insertion of R'^+ in a translocation (T) chromosome has little or no influence on the strong paramutagenic action of this allele in heterozygotes with R' on a normal chromosome (TR'^+/R'). The effects, under (i) and (ii), seemingly appear only after a lag of at least one generation following introduction of R' into a T chromosome. Such germinally transmissible changes in R' action, once induced in a TR'/R' plant, invariably persist for at least one generation following return of R' from a T to a structurally normal chromosome by crossing over. This fact excludes conventional position effect as an explanation of the phenomenon. For reasons not at present apparent, R' in coupling with a translocation (TR') is somewhat lower in pigment-producing action, and is more sensitive to paramutation in TR'^+/R' heterozygotes, when derived from TR'/R' (homozygous colored) than from TR'/r' (heterozygous colorless) plants. The evidence from these experiments suggests the occurrence of functional components of the chromosome much larger than individual loci.

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Induction and Acceleration of Gametogenesis in Flagellates by the Insect Hormone Ecdysone

Ecdysone, which is produced by the prothoracic glands of insects, was crystallized in 1954 by Butenandt and Karlson. It is a growth and differentiation hormone and is also referred to as the molting hormone.

The wood-feeding roach *Cryptocercus* is the host of a large assortment of protozoan flagellates: 9 families, 14 genera, and over 30 species. Each time this host molts its flagellates are induced to undergo sexual cycles, which they never undergo at any other time. An adult roach has no prothoracic glands, no ecdysone, no sexuality in its flagellates, and it does not molt. When it is injected with ecdysone, gametogenesis is induced in its flagellates, and the time required for induction depends, within certain limits, on the amount of ecdysone injected. 2000 units induces gametogenesis in four genera within 3 hours.

Under natural conditions the sexual cycles of some genera of these flagellates require 6, some 9, some 26, and some 40 to 50 days for completion. When fairly large amounts of ecdysone are administered to hosts lacking this hormone, the sexual cycles are greatly accelerated; those requiring 40 to 50 days for completion under natural conditions are all completed in experimental hosts in 7 days. If the experimental host is an intermolt nymph or an adult, it does not molt, yet its flagellates are induced to undergo sexual cycles.

Ecdysone does not induce zygotic meiosis, nor is its presence essential for fertilization. It is responsible for only the gametogenesis phase of sexual cycles.

L. R. CLEVELAND

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Strand Separation and Specific Recombination in Deoxyribonucleic Acids

When solutions of bacterial deoxyribonucleic acids (DNA) are denatured by heating to 100°C and then cooled, two different molecular states can be obtained in essentially pure form, depending on the choice of conditions. When the cooling is fast and the concentration of DNA below 40 µg/ml, the DNA is single stranded, unaggregated and has a molecular weight of about half that of the original DNA. This is essentially inactive in bacteriological transformation and is called denatured DNA. With slow cooling at 0.3 mM sodium ions and at concentrations of DNA near 20 µg/ml the DNA consists of recombined strands united by complementary base pairing over most of their length. This form has as much as 50 percent of its original transforming activity and is called renatured. These two forms can be clearly identified by differences in (i) absorbance-temperature curves, (ii) density, (iii) appearance in electron micrographs and (iv) hydrodynamic properties. The recombination has the concentration dependence of a bimolecular association. Wild-type DNA can replace homologous

DNA carrying genetic markers but non-homologous DNA has no effect on the extent of recombination. Thus the recombination is specific.

Density gradient experiments on N^{14} and N^{15} *Escherichia coli* DNA have shown the existence of hybrids in the DNA renatured from the mixture. Similarly, hybrids have been shown to form between the strands of bacteria that are closely related genetically. Thus the possibility of forming, by renaturation, heterozygous DNA molecules with different genetic markers or different chemical modifications in the two strands seems assured.

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Stress and Duodenal Ulcer

The concept that duodenal ulcers are usually caused by an abnormal hypersecretion of gastric juice of nervous origin is supported by the following evidence. (i) Duodenal ulcer patients usually secrete from 3 to 10 times more acid in the fasting, empty stomach at night than do normal people. (ii) If a hypersecretion of this degree is produced in experimental animals they regularly develop duodenal ulcers. (iii) If the vagus nerves to the stomach in duodenal ulcer patients are divided, the hypersecretion is abolished, and the ulcers usually heal.

Recently an alternative concept has been advanced to account for the role of physical and mental stress in duodenal ulcer. It is postulated that such stress stimulates the hypothalamus which in turn stimulates the anterior hypophysis to increased liberation of adrenocorticotrophic hormone (ACTH). This in turn causes increased production of cortisone. Cortisone, it is claimed, stimulates gastric secretion and causes duodenal ulcer. In this report evidence will be presented that ACTH, cortisone, and epinephrine do not stimulate gastric secretion and, furthermore, that physical and mental stress do not stimulate gastric secretion if the vagus nerves have been cut.

It is suggested that the stresses of modern life play an important role in duodenal ulcer by causing in some way an increased activity of the vagus nerves. This in turn causes an increase in stomach motility and an abnormal hypersecretion of acid gastric juice.

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Dissociation Energies of Gaseous Alkali Halides

The newly available data on gas imperfections of alkali halide vapors, together with entropies calculated from low temperature heat capacities, microwave determinations of internuclear distances, and infrared observations of vibrational frequencies were used to carry out a critical evaluation of all the vapor pressure data for the alkali halides. The resulting heats of sublimation were com-

bined with heats of formation to obtain dissociation energies of the gaseous alkali halides.

These experimental dissociation energies were compared with those calculated on the basis of a classical electrostatic model. Although reasonable agreement is obtained for alkali halides with large ions, when the polarizability of the ions is taken into account, alkali halides containing small ions and particularly the halides of lithium show substantial discrepancies.

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Morphological Model for Human Intelligence

The results from a factor-analytical approach to the investigation of abilities, particularly in the past 10 years, have revealed some 55 distinct dimensions of intellect. Consideration of their logical relationships has resulted in the organization of those abilities into a system known as the "structure of intellect."

The abilities are cross-classified according to three parameters—operations (five kinds), content (four kinds), and products (six kinds). These interacting classes can be represented geometrically by a rectilinear model. Vacant cells within the model forecast many abilities yet to be discovered, possibly bringing the number of intellectual factors to 120 or more.

The implications of this conception of intellect are numerous for general psychological theory, for aptitude testing, and for education. The kind of psychology implied emphasizes the individual as an agent for dealing with information. Two of the parameters of the model pertain to varieties of information, its sources, and the kinds of products that the individual makes of it. Aptitude testing can become far more analytical. The knowledge of new factors extends considerably the possibilities of assessment into areas of intellect previously neglected, for example, creative thinking and evaluation or judgment. Education may again put "training the mind" among its highest objectives. Furthermore, education will have a more enlightened basis for its operations of developing intellectual skills.

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Pleiotropism as Studied in Galactokinaseless Mutants of *Escherichia coli* K-12

As has been reported previously [H. M. Kalckar, K. Kurahashi, and E. Jordan, *Proc. Natl. Acad. Sci. U.S.A.* **45**, 1776 (1959)], the synthesis of galactose-1-phosphate uridyl transferase (which is low in the galactose-positive strains without addition of galactose) is independent of galactose in two strains with hereditary blocks in galactokinase. The following code is used with respect to inducibility and constitutivity of kinase (*K*) and

transferase (*T*). If the rate of enzyme activity in the absence of galactose is below 10 percent, "i" is used, below 1 percent, "ii," whereas rates above 20 percent of full constitutivity are denoted "c,i." Full constitutivity is represented by "c." Omission of these letters means that inducibility has not been tested. The galactose-positive wild type is accordingly $K^{+}T^{+}$; the galactokinaseless, $K^{-}T^{+}$. The latter strains can revert to galactose-positive in which galactokinase activity is restored (K^{+}) and at the same time *T* loses constitutivity, that is $K^{+}T^{-}$ or $K^{+}T^{+}ii$. From $K^{-}T^{+}$ has been isolated $K^{-}T^{+}i$, which readily reverts to $K^{+}T^{+}i$ (M. B. Yarmolinsky, H. Wiesmeyer, and E. Jordan, unpublished). It is also of interest that transduction of $K^{-}T^{+}$ to a transferaseless host ($K^{-}T^{-}$) yields a galactose-positive heterogenote [$K^{-}T^{+}i_{\text{LysR}}/K^{-}T^{-}$] which phenotypically is $K^{+}T^{+}ii$ [H. Wiesmeyer and M. B. Yarmolinsky, *Bacteriol. Proc.* (1960)]. As emphasized previously (H. M. Kalckar *et al.*), the galactokinase activity seems an irrelevant factor in determining $T^{+}i$; rather, true pleiotropism is involved or an alternative galactokinase is synthesized which results in the formation of an enzyme capable of destroying a naturally occurring cellular repressor for *T*. This possibility might be approached by obtaining a time curve for *T* right after the formation of the above-mentioned heterogenote, in case a $T^{+}i$ activity can be detected prior to a $T^{+}ii$.

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ELKE JORDAN

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Evaporation Resistances of Mixed Monolayers

In our previous paper [*Proc. Natl. Acad. Sci. U.S.A.* **45**, 1274 (1959)] we emphasized that the resistance which a monolayer spread on a water surface exhibits to evaporation is very sensitive to the presence of contaminants of lower resistance. We have accordingly investigated mixed monolayers of saturated long-chain hydrocarbon compounds where the head groups are COOH, or OH, or mixtures of COOH and OH. The logarithm of the resistance to evaporation is a linear function of the mol fraction of the components, if the polar head groups are the same. When the head groups are a mixture of COOH and OH, specific interactions are found.

When the C_{12} alcohol is contaminated with the C_{18} alcohol, the latter molecules are squeezed out of the monolayer on compression, so that above a surface pressure of about 14 dyne/cm the mixture behaves as though it were almost pure C_{18} alcohol. On the other hand, when C_{18} alcohol is added to C_{12} alcohol, the C_{12} alcohol is not squeezed out entirely on compression, and the evaporation resistance is lowered. These findings have a bearing on the selection of commercial samples of mixed alcohols for field studies on the reduction of evaporation of water from reservoirs.

VICTOR K. LA MER
GEOFFREY T. BARNES

Columbia University

Relation of Beet-Yellows Virus to Phloem Tissue

Beet-yellows virus induces the formation of inclusions in infected plants. These inclusions appear first in phloem tissue but later develop also in ground tissues outside the phloem and in the epidermis. Within the phloem, the inclusions develop in nucleate cells (phloem-parenchyma and companion cells) located next to mature enucleate sieve elements. No inclusions were seen in mature sieve elements themselves. The initial localization of inclusions and their subsequent spread suggest that the virus is translocated in mature enucleate sieve elements but multiplies outside these elements in nucleate cells, first in those of the phloem, later also outside the phloem. In its host-tissue relation the beet-yellows virus appears to be intermediate between the typical phloem-limited yellows viruses and the mosaic viruses, which are able to multiply in any tissue.

KATHERINE ESAU

University of California, Davis

Plasmotypes of Maize as Evidence of Cytoplasmic Diversity and Continuity within a Species

In *Zeae mays* three different types of cytoplasm have been found. These types are separated by their interaction with known genes to produce either normal or aborted pollen. These cytoplasmic differences have persisted without alteration during 15 generations of backcrossing and presumably have existed for very long periods of time. This raises two questions which have not yet been answered: Have these different plasma-types been derived from the different species that have presumably contributed to cultivated maize as we now know it? Or have the plasmagenes mutated since the species was established? This has an important bearing on the origin of cytoplasmic differences and the differentiation and separation of species.

D. F. JONES, H. T. STINSON, JR.,
UHENG KHOO

Connecticut Agricultural Experiment Station

Enzymic Mechanism of Increased Utilization of Glucose during Virus Multiplication in the Chorioallantoic Membrane of the Chick Embryo

The chorioallantoic membrane of the chick embryo responds with a marked increase of lactic acid accumulation in vitro, when virus multiplication takes place in this tissue. This increased glycolysis was previously found to be caused by an augmentation of glycolytic enzymes [E. Kun and M. H. D. Smith, *Proc. Soc. Exptl. Biol. Med.* **73**, 628 (1950); M. H. D. Smith and E. Kun, *Brit. J. Exptl. Pathol.* **34**, 1 (1954)]. Recent investigations have revealed that the increase of glycolysis occurs simultaneously with a net increase

of glucose utilization and an increased turnover of the "pentose cycle," as measured by CO_2 production from glucose-1-C¹⁴. Since the concentration of enzymes catalyzing the oxidation of hexose monophosphates does not increase [E. Kun, *Proc. Soc. Exptl. Biol. Med.* 83, 532, (1953)] in contrast to glycolytic enzymes, the enzymic mechanism of the increase in the hexose monophosphate pathway was further analyzed. It was found that the rate-limiting reaction in the rate of the pentose cycle was the reoxidation of reduced triphosphopyridine nucleotide (TPNH). The main oxidizing agent is pyruvate, and the enzyme catalyzing this oxidation is a lactate dehydrogenase of the chorioallantoic membrane. The synthesis of this enzyme in virus-infected membranes occurs simultaneously with an increase in the pentose cycle. A study of the properties of this enzyme revealed that one of the reactions catalyzed by it,



is inhibited by iodoacetate, in contrast to other lactate dehydrogenases. It is concluded that virus infection results in an activation of biosynthesis of glycolytic (and other) enzymes. The increase in the pentose cycle is explained by an increase of lactate dehydrogenase (TPNH-oxidizing enzyme) and by some change in the intracellular availability of DPN⁺, which otherwise inhibits the TPNH → pyruvate reaction. A possible implication of these findings in the mechanism of glycolysis in tumors is suggested.

ERNEST KUN, J. E. AYLING,
BENJAMIN V. SIEGEL

*University of California School
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found that such streams of air can retain their integrity even while deflected, thus making pure pneumatic amplifiers possible. Control of the streams is accomplished through the use of momentum interaction and boundary layer effects.

BILLY M. HORTON, R. E. BOWLES
Diamond Ordnance Fuze Laboratories

offer a microphotometric comparison of the spectra of Mars and the Moon. The curve showing the general decline in intensity of the Martian spectrum with wavelength closely resembles the absorption curve of nitrogen dioxide as published by several investigators. Further features of the curve are dips and depressions at wavelengths in close agreement with similar features observed in laboratory experiments on NO_2 with lower dispersion. As a check on this we have made observations at high dispersion on the absorption spectrum of NO_2 and find good agreement between our Martian and laboratory results. We conclude that oxides of nitrogen are present in the atmosphere of Mars and explain observed features of the Martian spectrum. The toxicity of these gases is evidence for the nonexistence of life on Mars.

C. C. KIESS

C. H. CORLISS

HARRIET K. KIESS

*National Geographic Society and
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Reappearance of Certain Structural Features of Native Collagen after Transformation

When soluble collagen is gently heated, configurational changes occur at a reproducible temperature which result in a sharp drop in optical rotation and intrinsic viscosity. Upon heating, tanned collagen fibers shrink and lose characteristic low angle x-ray reflections. On cooling, the physical properties partially return to their original values. Most significantly, the temperature of melting corresponds to the original temperature. These changes have been interpreted as evidence of a phase transition involving melting of the native helical structure to form random coils. Although the close agreement of temperatures for the initial and final products of the heating cycle indicates equivalence of the crystalline regions, reversion to short crystalline segments of the helix could give the results observed.

We have obtained electron micrographs of the particles present in calf-skin collagen solutions before heating, immediately after heating, and after subjecting the solution to 5°C for several hours. The characteristic rod-shaped collagen particles disappear completely on heating, being replaced by globular particles as seen in the electron micrographs. Upon cooling, significant numbers of rod-shaped particles (tropocollagen) reappear. We have also established the presence of highly ordered aggregates (segment long spacing or SLS type) after adding adenosine triphosphoric acid to heated and cooled collagen. The latter experiments show that the macromolecules can regain not only the original size and shape but also the rather precise side chain charge distribution necessary for SLS formation. Sedimentation, viscosity, and optical rotation studies confirm these results. Electron microscope studies of collagen fibrils crosslinked with formalin reveal that the characteristic striations (considered to be indicative of crystallinity) not only disappear with

Amplification by Fluid Stream Interaction

Recent experiments with interacting fluid streams show that it is possible to achieve an amplifying action by a stream-deflection method. Since this amplification does not require the use of moving parts, it is termed "pure fluid amplification." A "pure fluid amplifier" consists of a "power nozzle" which provides a high-velocity stream; two or more apertures to receive the kinetic energy of this stream; and one or more control nozzles to issue a control stream which impinges on the side of the high-velocity stream and thus varies the proportioning of the energy between the apertures.

A simplified treatment of a highly idealized, two-dimensional pure fluid amplifier shows that the power gain is of the order of $(2 L/W)^{3/2}$, where L is the distance from the region where the streams collide to the apertures and W is the width of the high-velocity stream at the apertures. Achieving a high power gain requires that the high-velocity stream maintain its integrity over a distance much greater than its width. In 1897 L. Mach published schlieren photographs showing that high-velocity jets of air maintain their integrity over considerable distances. It has been

Evidence for Oxides of Nitrogen in the Atmosphere of Mars

Observations of the spectrum of Mars, in July 1956, with the high dispersion of a concave-grating spectrograph, at the Slope Observatory on Mauna Loa, show a steady decline in intensity in the green, blue, and violet regions, relative to sunlight reflected from the moon. This effect is similar to that observed for Jupiter and Venus, and is attributed to absorption by nitrogen peroxide in the planet's atmosphere. As evidence for this inference we

vation of temperature, but are regained upon cooling.

The results of this study demonstrate that the reversal of the collagen helix \rightleftharpoons random coil transition is accompanied by restoration of the structure characteristic of the native state when proper heating and cooling schedules are followed.

ROBERT V. RICE

Mellon Institute

Mutation Frequency at Low Radiation Intensity

In the experiments that resulted in our finding that fewer mutations are produced by a given dose of chronic irradiation than by the same dose of acute irradiation, the radiation intensities adequately compared were approximately 90 r/wk and 90 r/min. The possible effect of a further reduction in dose rate is now being tested by measuring mutation frequency at specific loci in spermatogonia of mice irradiated at an intensity of 10 r/wk for total doses of 86 and 300 r (Cs^{137} source). The data already accumulated show that the mutation frequency at this dose rate is significantly higher than that in the controls ($P = 0.002$). Thus, there is no indication of a threshold dose rate (a dose rate below which no mutation occurs) even when the dose rate is reduced to this low level of 10 r/wk. This is, as far as we know, the lowest dose rate ever tried in laboratory mutation rate experiments with animals.

The mutation frequency at 10 r/wk is not significantly different from that obtained at 90 r/wk. Thus, there is, so far, not only no evidence for a threshold dose rate, but also no indication that further lowering of radiation intensity will result in any further reduction in mutation frequency.

W. L. RUSSELL, ELIZABETH M. KELLY
Oak Ridge National Laboratory

Phonetic Typewriter

The factors involved in the development of a phonetic typewriter are as follows: the particular form in which the words are typed; identification of the analyzed sounds; encoding, coding, and decoding of speech sounds for the operation of the actuating mechanism; and the design of the mechanism for actuating the typewriter. The system for analyzing and separating the sounds of speech constitutes one of the basic elements for the conversion of speech into the typed page. Since a sound wave may be completely described in terms of the amplitude and frequency of the components and time, the system for identifying the sounds of speech is based upon frequency, amplitude and time. The speech identifier includes (frequency, amplitude, and time) analyzers, compensators, correlators, and normalizers.

HARRY F. OLSON

RCA Laboratories

Beach Rock Investigations

Investigations in the Lesser Antilles and Puerto Rico lead to the conclusion that beach rock consists of beach materials cemented by calcite. Cementation is initiated in the vicinity of the water table under the beach. Exposure results from removal of overlying sediments, and quite commonly occurs intermittently, producing successive bands of beach rock. Each band indicates the position of a former beach. Divergent trends of these bands in some places provide a record of stages in the development of coastal outlines during the 4500-year interval of the existing still-stand of ocean levels. Outer bands of beach rock are commonly in process of transformation into reefs. After the poleward limits of beach rock along the coasts of the major oceans have been ascertained, it may be possible to state a climatic value indicating the length of season of warmth necessary to permit beach rock formation. If, upon investigation, it is found that considerable numbers of fringing reefs are anchored on beach rock, it may be possible to apply the climatic formula to one or more of the halting stages associated with the last general rise of sea level.

RICHARD J. RUSSELL

Louisiana State University

Phase Transition of a Superconductor

The Bardeen-Cooper-Schrieffer model of a superconductor is one of the few systems whose partition function can be evaluated in a closed form such that the phase transition is exhibited in a mathematically rigorous manner. This derivation (Bogoliubov, Zubarev, Tserkovnikov) implies certain tacit assumptions which call for a critical discussion. Another question is: How sensitive is the theoretical result—that is, the character of the phase transition—to the way one chooses to "reduce" the Hamiltonian, namely to split it into a main part that is treated rigorously, and perturbations which are either shown to have no effect or, otherwise, are deliberately neglected on the basis of plausibility arguments? As an example it will be pointed out that inclusion of the lowest order self-energy (or exchange Coulomb energy, or both) in the reduced Hamiltonian changes the character of the transition from second-order to first-order. Numerically, one may expect the heat of condensation to be very small, yet it might be detectable in very precise measurements.

G. WENTZEL

University of Chicago

Scale Size of Mechanisms for Solar Modulation of Cosmic Ray Intensity

The galactic cosmic radiation undergoes large changes in intensity and spectrum at the earth as a consequence of electromagnetic phenomena of solar origin. Over the 11-year solar activity cycle, there is

an intensity decrease of a factor 2 to 4 at solar activity maximum. On a shorter time scale, there are intensity decreases of 10 to 50 percent which take place in the order of hours—the Forbush decreases. To investigate the magnitude of the latter phenomenon in space, experiments have been performed with cosmic ray detectors on the earth and in the satellite Explorer VI. A Forbush decrease has been observed simultaneously at the earth and at a distance of more than 7 earth radii. Thus the scale size for the electromagnetic fields producing this effect is much greater than this distance, and it is unlikely that geocentric models for these fields account for the observations.

Pioneer V, recently launched in an orbit about the sun, also provides information on the magnitude of the region in interplanetary space over which the 11-year depression of cosmic ray intensity persists.

J. A. SIMPSON, C. Y. FAN,
P. MEYER

University of Chicago

Molecular Reorientation as Unifying Principle Underlying Cellular Selectivity

One of the most fundamental properties of cells is their discriminatory capacity for admitting or excluding selectively specific physical stimuli, chemical agents, and exchanges with fellow cells in their environment (for example, in food ingestion, drug reactions, hormone responses, parasitic infections, permeability, immune reactions, fertilization, virus reproduction, nerve excitation, cell-to-cell interactions in development and tissue repair). Some of these phenomena have been interpreted in terms of interlocking molecules of matching configuration. But the relation between the specificity and the dynamics of such interactions (transfer of substance and energy, current flow, cementing, and so forth) has remained undefined.

As a step toward a unitary concept, a "dualistic" hypothesis is herewith presented, based on the following assumptions. (i) A major fraction of the cell surface is occupied by a network of filiform macromolecules in essentially planar (surface parallel) array, barring substance passage ("barrier position"). (ii) Certain of these molecules have end groups of specific configuration as selective acceptors for complementary groups. (iii) Carriers of complementary end groups approaching the cell from the environment attract and combine with the matching surface groups and thereby right the respective molecules from tangential into radial ("open-gate") positions. (iv) For molecules with an axial (length : diameter) ratio of 100, this reorientation implies the uncovering of 99 percent of a formerly covered surface site; in other words, the opening of local "leaks" or "pores" as channels for secondary outflow or inflow across the surface. (v) Local electrostatic disturbances produce surface "leaks" in similar, but unspecific, fashion.

According to this concept, specific molecular interactions at the surface serve merely to unlock less specific and dynamically more potent transport and transmission mechanisms.

PAUL WEISS

Rockefeller Institute

Effects of Ribonucleic Acid on Mouse Ascites Cells

Intramuscular injection of Nelson's mouse ascites cells resulted in the formation of solid tumors with frequency of 96 percent in 146 Swiss mice. When these cells were incubated with RNA from calf liver, tumors developed in less than 10 percent of 154 mice. Electron microscopy and vital staining revealed no apparent difference between cells of the RNA-treated and untreated series. Both series were capable of incorporating *dL*-leucine-1-C¹⁴ into protein, although the liver-RNA treated cells were less active than the control. However, the fraction of TCA protein that cross-reacted with antiserum against bovine serum albumin was 3 to 7 times more radioactive in the experimental series than in the control. It appears that TCA protein synthesis of the liver-RNA incubated cells diminishes while their ability for synthesizing specific protein increases.

Commercial yeast RNA was also used as control for the incorporation studies. The pattern of the incorporation was almost the same as that in the untreated ascites cells.

Ascites tumor cells treated with RNA from solid tumors developed tumors with frequency of 71 percent in 24 mice. This finding helps rule out the possibility that the inhibitory activity of liver RNA is associated with contaminating substances introduced during the isolation procedure. When trypsinized kidney cells treated with tumor RNA were injected intramuscularly into the thigh, solid tumors developed at the site of injection in three of five mice. This was repeated three times, and four tumors appeared in 15 mice injected.

M. C. NIU

Rockefeller Institute

Sensory and Motivating Properties of Taste Stimuli

The sense of taste may be studied from two points of view. One emphasizes the processes involved in receptor stimulation and the resulting activity in the sensory nerves and central nervous system. The other pertains to the behavioral consequences of such receptor-neural activity. In particular, taste stimuli motivate strong unlearned preference or aversion reactions in animals and humans. The present report brings together data from both points of view.

The physiological properties of the sense of taste were studied in a variety of animals by recording the electrical activity in the chorda tympani nerve from the tongue and in the medullary and thalamic sensory relays for taste. Both at the

peripheral and subcortical levels, physiological activity is a rising function of stimulus intensity. Such measures were then correlated with results of studies utilizing preference and other behavior methods. When adequate account is taken of the role of post-ingestion factors, the behavioral response to certain positive stimuli, like sucrose, is directly related to the strength of the sensory discharge. A similar relation holds in the case of aversive stimuli. But in still other instances, behavior may be a duplex function of the intensity of the sensory input—that is, the stimulus is attractive at low, but aversive at higher, intensities.

These phenomena will be discussed in relation to the general problem of the motivating effects of sensory stimulation in the control of behavior.

CARL PFAFFMANN

Brown University

Two Fundamental Errors Widely Held in Fusion Research

Two commonly made assumptions lead to the belief that a sufficiently strong magnetic field may be impressed upon a container made of ordinary materials which cannot withstand more than a few thousand degrees centigrade and that the field will allow the container to hold a plasma gas at a temperature high enough for a thermonuclear reaction, more than 10° degrees, to take place and yield its energy for a useful purpose.

One error [L. Spitzer, Jr., *Physics of Fully Ionized Gases* (Interscience, New York, 1956), p. 20, approximation 1] comes from neglecting the convective change of momentum, $s\vec{v} \cdot \nabla \vec{v}$ in the total change of momentum of the gas,

$$s \frac{d\vec{v}}{dt} = s \frac{\delta\vec{v}}{\delta t} + s\vec{v} \cdot \nabla \vec{v}$$

The other error [L. Spitzer, Jr., *op. cit.*, p. 83 (5-34 and 5-35); p. 21 (2-12)]. The velocity \vec{v} in question is not that of a Lorentz gas.] comes from underestimating the force exerted by the electrons upon the ions as they move through the latter. This force leaves the moment of momentum of the ions practically unchanged during their motion. At the same time it negative forces the electrons to move radially with the ions. Thus the magnetic field has little effect on the rate of the loss of ionization to the walls.

The way to utilize nuclear energy is by means of the ionic centrifuge [J. Slepian, *Science* 129, 1289 (1959)] with the cylinder at the same voltage as the central arc and the end plates at a very high positive potential.

JOSEPH SLEPIAN

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Fixation of Nitrogen by Cell-Free Extracts from Microorganisms

Various methods have been applied to several types of organisms in past attempts to obtain cell-free extracts capable of fixing nitrogen. The experiments yield-

ed some positive results, but fixation has been limited and inconsistent. Carnahan et al. [*Biochim. et Biophys. Acta* 38, 188 (1960)] reported that cells of *Clostridium pasteurianum* dried at 40° to 50°C yield a soluble preparation capable of consistent and readily measurable nitrogen fixation in the presence of pyruvate. We have confirmed their results and have successfully extended the same method to *Rhodospirillum rubrum*. Fresh cells of several species of blue-green algae have also given active cell-free preparations after disruption by sonic oscillation. In a representative experiment with a preparation from *C. pasteurianum* exposed to N₂ for 0.5 to 4 hours, the total nitrogen carried 0.72 to 1.58 atom percent N¹⁵ excess. Preparations from *R. rubrum* incubated with N₂ for 2 hours had 0.54 to 3.08 atom percent N¹⁵ excess in their total nitrogen. Cell-free extracts from fresh cells of the blue-green algae *Mastigocladus laminosus* accumulated from 0.017 to 0.247 atom percent N¹⁵ excess in 90 minutes. A preparation from *C. pasteurianum* yielded ammonia with 52 atom percent N¹⁵ excess after 3 hours; this high level of N¹⁵ in the ammonia recovered supports the ammonia hypothesis of nitrogen fixation. Although a few positive results have been observed with preparations from two other nonphotosynthetic anaerobic nitrogen-fixers, *Aerobacter aerogenes* and *Bacillus polymyxa*, these preparations to date have been less satisfactory than those obtained with the *Clostridium* and the photosynthetic organisms.

P. W. WILSON, R. H. BURRIS

University of Wisconsin

Factors Causing a High Frequency of Mice Having the XO Sex-Chromosome Constitution

Earlier work at this laboratory demonstrated that XO mice are fertile females. Incidence of primary XO ranges from 0.4 to 0.8 percent in various stocks. XO animals were postulated to arise either from nondisjunction of the sex-chromosomes in gametogenesis or from events occurring after fertilization. Because there seemed some evidence in support of the second hypothesis (for example, nonrandom distribution of primary XO's among litters and sibships), an experiment was undertaken to test whether the frequency of XO could be increased by irradiation shortly after fertilization. Sex-linked markers were used so that each XO animal could be recognized and the X identified as macroclinos or microclinos.

The frequency of XO among animals given 100 r within a few hours after fertilization (pronuclear stage) was 9 percent. This is significantly higher than earlier spontaneous incidences and, moreover, includes the first cases of microclinos XO observed in the mouse. The results indicate that XO's can originate after fertilization. At present, however, some doubt remains whether the high incidence in the experimental group is due to irradiation, since the frequency of XO in the small control group of this experiment is

also unusually high. If radiation is not the cause, the most likely alternative is genetic interaction, that is, the particular combination of stocks used (each parent strain by itself yields the usual low spontaneous incidence). Here too, however, the high incidence of XO would be due to some event connected with, or following, fertilization.

LIANE BRAUCH RUSSELL
CLYDE L. SAYLORS

Oak Ridge National Laboratory

Shortening of the Life Span by Ionizing Radiation: Possible Relation to Accelerated Aging

The life span is reduced by whole-body irradiation in rodents, the extent of life-shortening varying in relation to the amount of radiation absorbed, the linear energy transfer of the radiation, and the dose rate. The reduction in longevity is not attributable to any one cause of death but is correlated with premature onset of neoplastic and non-neoplastic diseases otherwise associated with senescence. Not all age-dependent changes are similarly affected, however, the incidence and severity of various diseases differing with the radiation dose. Hence, irradiation does not simply advance the onset of senescence, unless it is inferred that the aging of different organs is advanced to different degrees. Although certain effects of radiation resemble those of natural aging, further study will be required to disclose whether these outward resemblances imply common biologic mechanisms. Certain delayed somatic radiation effects have been noted in man, but radiation-induced shortening of the life span comparable to that observed in animals has yet to be documented in human populations.

A. C. UPTON
A. W. KIMBALL

Oak Ridge National Laboratory

Crustal Structure in the United States from a Study of Gravity and Seismic Data

One result of the IGY seismic program was the verification of the fact that the composition of the earth's crust is far from uniform and that in some areas it may be almost entirely of basic rock composition and in other areas of granitic rock composition. These, however, constitute exceptions and in general the crust has a layered structure defined by variations in velocity in the range 5.4 to 7.6 km/sec corresponding on the basis of available data to density values in the range 2.65 to 3.1 gm/cm³. A study of the relation of crustal seismic data to Bouguer and isostatic anomaly values suggests that in general positive isostatic anomalies are related to a high density, thick crust rather than a lack of isostatic equilibrium and an abnormally thin crust. In the case of negative isostatic anomalies a general

case can not be established because of the large gravity contribution of surficial deficiencies in mass associated with sedimentary basins. Because compaction and cementation factors as well as the type of sediment in deep basins is usually not well known, a reliable correction for the geologic factor in such areas can seldom be made. The common association of negative anomaly values with areas of granitic rocks, however, suggests that these rock extend to considerable depth and that the mean crustal density is less than that for a crust of normal composition having the same Bouguer gravity anomaly. The departures from what might be regarded as normal crustal values are less for negative isostatic anomaly areas than would be theoretically predicted, but that for positive isostatic anomaly areas is about that to be expected theoretically. Using these empirical relations and a combination of Bouguer and regional isostatic anomaly maps, one can predict with a reliability of about 10 percent variations in crustal thickness. This has been done for the United States and a contour map for the elevation of the Mohorovičić discontinuity below sea level has been prepared.

G. P. WOOLLARD

University of Wisconsin

High Effectiveness of Fast Neutrons in Inducing Minute Deletions

A 1500-rad dose of neutrons derived from U²³⁵ fission, delivered in 1 hour to mature *Drosophila* sperm 1 to 2 days before their ejaculation, was found to induce recessive sex-linked lethals at approximately the same frequency (10 to 11 percent) as 4000 r of "acutely" delivered x- or Co⁶⁰ gamma-radiation applied to spermatozoa of the same stage or to late oocytes. This result confirms our earlier work, setting the neutron RBE (relative biological effectiveness) at approximately 2.5 for such lethals. In our present work minute deletions of several types were also detectable. Significantly more of them were found to be induced by the given neutron than by the x-ray or gamma exposure, the RBE for the deletions being of the order 3.5 to 5. Similar 4000 r x-ray or gamma exposure of oögonia resulted, as in our earlier work, in a frequency of lethals only a seventh to a fifth as high as from the irradiated spermatozoa or oocytes, while the frequency of minute deletions appeared even more reduced. A similar result for deletions was obtained from spermatogonia irradiated with 4000 r x- or gamma-radiation in genetic set-ups that protected affected spermatogonia from the otherwise lethal hypoploidy caused by such deletions. Again, the 1500-rad neutron treatment of gonial stages resulted in frequencies of deletions distinctly higher than after 4000 r x- or gamma-irradiation. The higher effectiveness of the more crowded ionizations resulting from neutron exposure in inducing the two nearby breaks re-

quired for minute deletions demonstrates the shortness of the path from site of ionization to resultant break.

This work was supported by grant AT (11-1)-195 from the Atomic Energy Commission. We acknowledge with thanks Dr. H. J. Curtis's and Dr. D. Steffensen's cooperation in arranging for, the neutron and gamma treatments and dosimetry at Brookhaven National Laboratory.

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I. I. OSTER

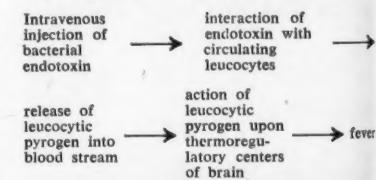
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H. J. MULLER

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Studies on Experimental Fever with Particular Reference to the Pathogenetic Role and Chemical Properties of Leucocytic Pyrogen

When fever is produced experimentally in rabbits by the intravenous injection of bacterial endotoxin, the injected endotoxin causes a prompt leucopenia and is rapidly cleared from the blood stream. In its place there appears an endogenous pyrogen which acts directly upon the thermoregulatory centers of the brain. The biological properties of the endogenous factor are different from those of the injected endotoxin but are indistinguishable from the properties of the pyrogen in normal polymorphonuclear leucocytes. Since the incubation of leucocytes with endotoxin in vitro not only inactivates the endotoxin but also causes the release of leucocytic pyrogen into the surrounding medium, and since the endotoxin, when injected in the usual pyrogenic dose, acts indirectly, rather than directly, upon the thermoregulatory centers, it is assumed that endotoxin fever is produced by the following mechanism:



An endogenous pyrogen, similar to if not identical with leucocytic pyrogen, has also been demonstrated in blood of rabbits with fever produced by: (i) tuberculin hypersensitivity; (ii) intravenous injection of influenza virus, and (iii) active bacterial infection.

Because of the central role which leucocytic pyrogen appears to play in each of these forms of experimental fever, attention has been turned to the isolation and chemical characterization of the leucocytic factor. Its pyrogenicity has been shown to depend upon an essential protein moiety.

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SCIENCE, VOL. 131

Meetings

Macromolecular Complexes

The Society of General Physiology held its 14th annual meeting at the University of Illinois, Urbana, 7, 8, and 9 September 1959. Contributed papers were presented on these three days, and on 9 September a joint session was held with the American Physiological Society, concurrently holding its fall meeting in Urbana.

The annual symposium of the Society of General Physiology, on the origin and role of complex macromolecular aggregates in development, was organized by M. V. Edds, Jr., and was held in connection with the annual meeting. Proceedings of this symposium will be published by the Ronald Press. This will be the sixth symposium volume published under the auspices of the society.

D. F. Waugh set the mood of the symposium in the first paper. He discussed several aspects of macromolecular interaction which may be involved in the formation of structures visible with the electron microscope, and in functions which begin to emerge at this level. He contended that the specificity of interaction between large molecules can be largely "understood on the basis of short-range interactions between appropriately placed submolecular groups of atoms having different interaction characteristics." In some cases, however, short-range forces seem insufficient to account for the observations. The presence of large amounts of water in some biological systems, for example, poses special problems which have set off speculations about the structure of water, and about long-range forces. None of these speculations, however, has led so far to important increases in our understanding of how "structure is developed and of the relationship between structure and function."

A. Hodge then discussed recent studies which confirm and extend previous hypotheses advanced by F. O. Schmitt and his co-workers concerning the nature of collagen macromolecules and the interaction mechanisms whereby they become aggregated into fibrils. Particularly cogent new evidence was adduced in support of the idea that

the native fibril is composed of tropocollagen units packed in a staggered array, with each unit displaced longitudinally 0.25 of its length in relation to neighboring units. The probable importance of "end chains" in the formation of collagen aggregates was supported by the observation that proteases split a terminal, tyrosine-rich polypeptide from the collagen macromolecule and thereby prevent the formation of end-to-end linkages.

M. J. Glimcher reviewed his general concept of the mechanisms of calcification, with particular reference to the role of the collagen fibril in providing sterically matching sites for crystals of hydroxyapatite. He also considered a substantial body of new evidence bearing on the question of which of the amino acid side chains constitute or contribute to these sites. The epsilon amino groups of lysine and hydroxylysine appear to be of primary importance, as revealed especially by experiments in which the capacity of collagen to initiate mineralization was reversibly inhibited by blocking these groups with FDNB.

The second session of the symposium was devoted to lamellar and fibrous systems. H. Fernández-Morán reviewed the major structural features of representative lamellar systems in the myelin sheath and in photoreceptors and described the advances made possible by the use of improved low-temperature preparative methods. He also discussed the molecular organization of lamellar systems in relation to general concepts of energy transfer processes now emerging from biochemical and biophysical studies.

J. J. Wolken next took up the chloroplast, including its lamellar structure and its molecular organization as a protein-lipid-pigment complex. Citing evidence collected by several techniques, he considered the chloroplast developmentally, structurally, and functionally. Turning to the mitotic spindle as an example of a fibrillar aggregate, H. A. Went described his immunological analyses of the origin of spindle components. His results suggest that the mitotic apparatus contains only two antigens, and that these both occur in the unfertilized egg.

The final session of the symposium dealt with fibrous and particulate aggregates in plants. J. A. Bergeron discussed the submicroscopic chromatophores involved in photosynthesis in the purple sulfur bacterium *Chromatium*. He considered the chromatophore as a pigmented particle with specific physicochemical properties and chemical composition; as the structural and functional unit of photophosphorylation; and as the photochemical organelle. The various data presented were used to construct a model of the ultrastructure of the chromatophore as a complex of protein, lipid, and pigment molecules.

W. J. Nickerson drew attention to the drastic extraction methods previously used to separate the polysaccharide fraction of the yeast cell wall, pointing out that they had obscured both "the importance of protein-polysaccharide complexes as the macromolecular fibers of the cell wall fabric" and the natural structure of the polysaccharide. New data were given for cell walls cleaned by differential centrifugation. The isolation of three protein-polysaccharide complexes was reported, along with their chemical, physicochemical, immunological, and electron-microscopic properties.

Lastly, R. D. Preston undertook "to inquire how far present evidence will allow us to go in defining" cellulose-protein complexes involved in the formation and final structure of the cell walls of higher plants. After reviewing the evidence that polysaccharides play a role in stabilizing associated collagen, and that cellulose-protein complexes occur in mammalian tissue, he considered the celluloses of plant cell walls, giving special attention to wall-cytoplasmic interactions. He concluded that while no direct biochemical evidence exists for cellulose-protein complexes, there is good indirect evidence. Further studies of regions in or near the cytoplasmic surface are urgently needed.

The annual business meeting of the society was held 8 September. Newly elected officers were William D. McElroy, president; John Buck, vice president; James W. Green, secretary; and Edward G. Boettiger, treasurer. William Arnold and Harry Grundfest were elected for 2-year terms as councilors. Thirty new members were elected.

The 15th annual meeting of the Society of General Physiologists will be held 6-8 September 1960, at Woods Hole, Mass.

M. V. EDDS, JR.

Department of Biology, Brown University, Providence, Rhode Island

J. W. GREEN

Department of Physiology, Rutgers University, New Brunswick, New Jersey

Forthcoming Events

May

21-22. Society for Economic Botany, 1st annual, Lafayette, Ind. (Q. Jones, New Crops Research Branch, Beltsville, Md.)

22. Maryland Acad. of Sciences, Baltimore. (J. W. Easter, Owings Mills, Md.)

22-26. Air Pollution Control Assoc., 53rd annual, Cincinnati, Ohio. (C. W. Gruber, 2400 Beekman St., Cincinnati 14)

22-26. Oil and Gas Power Conf., Kansas City, Mo. (D. B. MacDougall, ASME, 29 W. 39 St., New York 18)

23-25. American Soc. for Quality Control, annual conv., San Francisco, Calif. (W. P. Youngclaus, Jr., ASQC, 161 W. Wisconsin Ave., Milwaukee 3, Wis.)

23-25. National Telemetering Conf., Santa Monica, Calif. (A. F. Denham, American Rocket Soc., 925 Book Bldg., Detroit 26, Mich.)

23-25. Technical Assoc. of the Paper and Pulp Industry, Chicago, Ill. (J. Winchester, TAPPI, 155 E. 44 St., New York 17)

23-26. Design Engineering Conf., New York, N.Y. (D. B. MacDougall, ASME, 29 W. 39 St., New York 18)

23-28. American College of Cardiology, 9th annual conv., Indianapolis, Ind. (G. F. Greco, ACC, 114-08 Linden Blvd., Ozone Park 16, N.Y.)

23-28. Instruments, Electronics, and Automation Exhibition, Olympia, London, England. (Industrial Exhibitions Ltd., 9 Argyll St., London, W.I., England)

23-28. International Ceramic Cong., 7th, Great Britain. (G. N. Hodson, Organizing Council, c/o Hatherware Ltd., Loughborough, England)

23-28. International War—Prophylaxis Cong. for Physicians, Noordwijk ann Zee, Netherlands. (M. Knap, 46 Schubertstraat, Amsterdam, Netherlands)

24-29. International Council for Bird Preservation, 12th cong., Tokyo, Japan. (Miss P. Barclay-Smith, British Museum (Natural History), Cromwell Rd., London, S.W.7, England)

25-26. Refractory Metals and Alloys, symp., Detroit, Mich. (E. O. Kirkendall, AIIIE, 29 W. 39 St., New York 18)

25-5. International Federation for Housing and Town Planning, cong., Puerto Rico. (IFHTP, Park Hotel, Molenstraat 53, The Hague, Netherlands)

26-27. Psychophysiological Aspects of Space Flight (School of Aviation Medicine, USAF Aerospace Medical Center), symp., San Antonio, Tex. (J. Harmon, Southwest Research Inst., 8500 Culebra Rd., San Antonio 6)

26-28. Society of Naval Architects and Marine Engineers, spring, Washington, D.C. (W. N. Landers, SNAME, 74 Trinity Pl., New York 6)

29-4. American Soc. for Horticultural Science, 8th annual of Caribbean Region, San Juan, Puerto Rico. (E. H. Cáceres, Londres 40, O.E.E., Mexico 6, D.F.)

29-5. International Commission on Irrigation and Drainage, 4th cong., Madrid, Spain. (D. Diaz-Ambrona, Comité Nacional Español, c/o Ministerio de Obras

Públicas, Agustín de Bethencourt, 4, Madrid, Spain)

30-1. American Gynecological Soc., Williamsburg, Va. (A. A. Marchetti, Georgetown Univ. Hospital, Washington 7)

30-2. American Orthopaedic Assoc., Hot Springs, Va. (L. R. Straub, 535 E. 70 St., New York 21)

30-3. Asian-Pacific Cong. of Cardiology, 2nd, Melbourne, Australia. (A. E. Doyle, Alfred Hospital, Melbourne, S.1, Victoria, Australia)

30-3. Fibre Science, annual conf., London, England. (A. W. Bennett, Textile Inst., 10 Blackfriars St., Manchester 3, England)

30-4. Reactivity of Solids, 4th intern. symp., Amsterdam, Netherlands. (Ir. G. van Gijn, Secretary, 4th Intern. Symp. on the Reactivity of Solids, Technisch Hogeschool, Eindhoven, Netherlands)

June

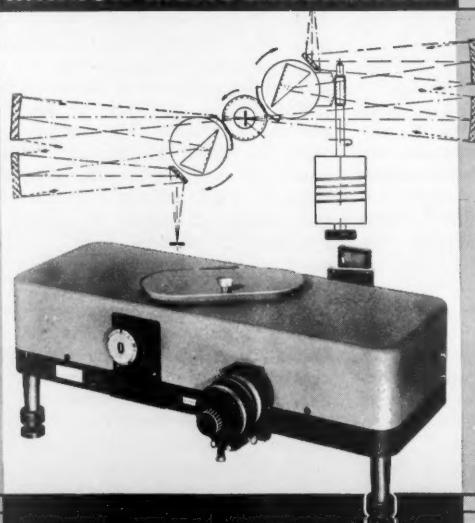
1-3. Instrumental Methods of Analysis, annual symp., Montreal, Quebec, Canada. (W. H. Kushnick, Instrument Soc. of America, 313 Sixth Ave., Pittsburgh 22)

1-3. Radar Symp., 6th annual, Ann Arbor, Mich. (W. A. Blikken, Willow Run Laboratories, P.O. Box 2008, Ann Arbor)

1-4. American Assoc. of Bioanalysts and California Assoc. of Clinical Laboratories, annual, San Francisco, Calif. (Mrs. M. K. Higgins, 75 Buena Vista Ave., San Francisco 17, Calif.)

1-5. Irrigation and Drainage, 4th intern. cong., Madrid, Spain. (D. Diaz-Ambrona,

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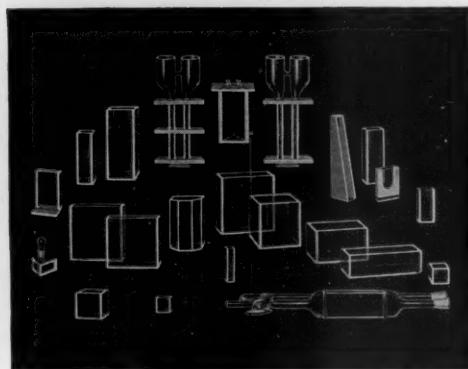
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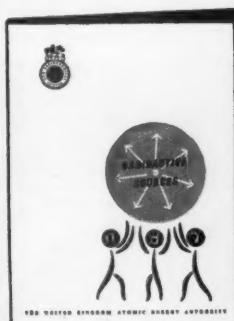
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2-4. Drugs Affecting Lipid Metabolism, intern. symp., Milan, Italy. (S. Garattini, c/o Institute of Pharmacology, Via del Sarto 21, Milan, Italy)

3-8. Pan American Medical Women's Alliance, 7th cong., San Juan, Puerto Rico. (Mrs. S. D. Rosekrans, 504 Newett St., Nullsville, Wis.)

5-8. Special Libraries Assoc., 51st annual, Cleveland, Ohio. (B. M. Woods, SLA, 31 E. 10 St., New York 3)

5-9. American Soc. of Mechanical Engineers, summer annual and aviation conf.,

Dallas, Tex. (L. S. Dennegar, ASME, 29 W. 39 St., New York 18)

5-9. World Power Conf., Madrid, Spain. (D. J. Pérez, Pozuelo, Spanish National Committee, General Pardinas, 55, Madrid, Spain)

5-10. National Conf. on Social Welfare, annual, Atlantic City, N.J. (Natl. Conf. on Social Welfare, 22 West Gay St., Columbus 15, Ohio)

5-14. XXV Cold Spring Harbor Symp. on Quantitative Biology, Cold Spring Harbor, N.Y. (A. Chovnick, Biological Laboratory, Long Island Biological Assoc., Cold Spring Harbor)

6-8. Protein Structure and Function, 13th symp. in biology, Upton, N.Y. (D.

E. Koshland, Jr., Dept. of Biology, Brookhaven National Laboratory, Upton, N.Y.)

6-10. International Conf. on Live Poliovirus Vaccines, Washington, D.C. (Secretariat, Pan American Health Organization/World Health Organization, 1501 New Hampshire Ave., NW, Washington 6, D.C.)

7-11. Microwave Tubes, intern. cong., Munich, Germany. (Nachrichtentechnische Gesellschaft im VDE (NTG), Frankfurt-Main, Osthafenplatz 6, Germany)

7-13. Dosimetry in Health Physics, symp., Vienna, Austria. (International Atomic Energy Agency, 11 Kärntner Ring, Vienna 1, Austria)

7-15. Partial Differential Equations and Continuum Mechanics, intern. conf., Madison, Wis. (R. E. Langer, Mathematics Research Center, U.S. Army, Univ. of Wisconsin, Madison 6)

8-9. Selenium in Nutrition, conf., Ithaca, N.Y. (K. C. Beeson, U.S. Plant, Soil, and Nutrition Laboratory, Ithaca, N.Y.)

8-10. Canadian Federation of Biological Societies (Canadian Physiological Soc., Pharmacological Soc. of Canada, Canadian Assoc. of Anatomists, Canadian Biochemical Soc.), 3rd annual, Winnipeg, Manitoba. (E. H. Bensley, Montreal General Hospital, 1650 Cedar Ave., Montreal 25, P.Q.)

8-11. National Soc. of Professional Engineers, annual, Boston, Mass. (P. H. Robbins, NSPE, 2029 K St., NW, Washington 6)

8-12. American College of Chest Physicians, Miami Beach, Fla. (M. Kornfeld, 112 E. Chestnut St., Chicago 11, Ill.)

9-10. American Geriatrics Soc., Miami Beach, Fla. (R. J. Kraemer, 2907 Post Rd., Warwick, R.I.)

9-10. Canadian Inst. of Food Technology, 3rd annual conf., Winnipeg, Manitoba. (W. J. Eva, Box 846, Winnipeg, Manitoba)

9-11. Acoustical Soc. of America, Providence, R.I. (W. Waterfall, ASA, 335 E. 45 St., New York 17)

9-11. Endocrine Soc., Miami Beach, Fla. (H. H. Turner, 1200 N. Walker, Oklahoma City 3, Okla.)

9-11. National Speleological Soc., annual, Carlsbad, N.M. (G. W. Moore, U.S. Geological Survey, Menlo Park, Calif.)

9-12. American Medical Women's Assoc., Miami Beach, Fla. (Mrs. L. T. Majally, 1790 Broadway, New York 19)

9-12. American Rheumatism Assoc., annual, Hollywood-by-the-Sea, Fla. (F. E. Demartini, Presbyterian Hospital, 622 W. 168 St., New York 32)

9-12. American Therapeutic Soc., Miami Beach, Fla. (O. B. Hunter, Jr., 915 19 St., NW, Washington 6)

10-12. American College of Angiology, Miami Beach, Fla. (A. Halpern, 11 Hampton Court, Great Neck, N.Y.)

10-12. American Electroencephalographic Soc., Boston, Mass. (G. A. Ulett, 1420 Gratten St., St. Louis 4, Mo.)

10-12. Society for Biological Psychiatry, Miami Beach, Fla. (G. N. Thompson, 2010 Wilshire Blvd., Los Angeles 57, Calif.)

11. American Acad. of Tuberculosis Physicians, Miami Beach, Fla. (G. P. Bailey, P.O. Box 7011, Denver 6, Colo.)

11-12. American Diabetes Assoc., Miami Beach, Fla. (J. R. Connelly, 1 E. 45 St., New York 17)

(See issue of 22 April for comprehensive list)

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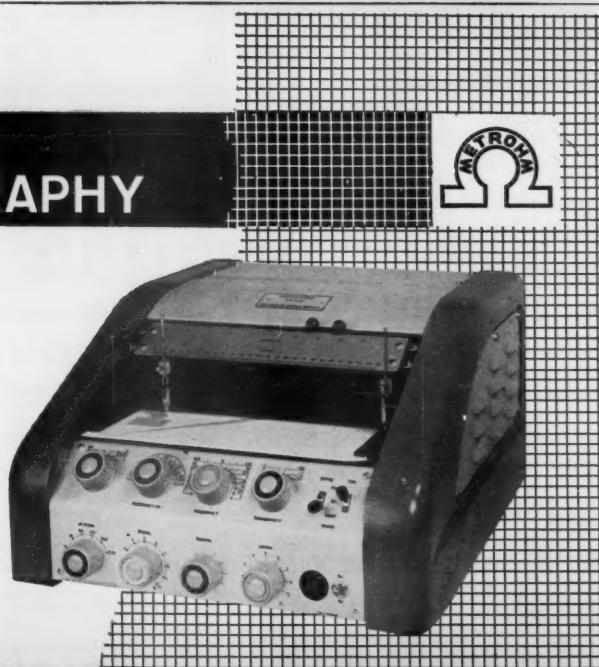


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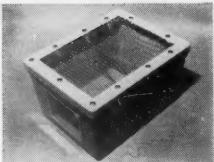
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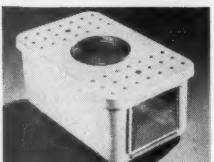
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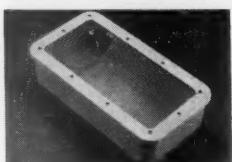
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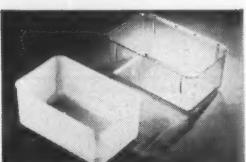
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■ PROXIMITY TRANSDUCER measures small mechanical displacements at frequencies from 0 to 20,000 cy/sec. Calibration can be performed under dynamic conditions; a micrometer head on the transducer is calibrated directly in 0.0001-in. units. Output is designed for cathode-ray-tube display. (Photocon Research Products, Dept. Sci480, 421 N. Altadena Dr., Pasadena, Calif.)

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(a) **Pharmacology/Physiology** Ph.D.; faculty of leading medical schools; extensive publications on CNS, cardiovascular, respiration, shock, tension research; available for academic or research appointment. (b) **Zoology** Ph.D., human anatomy minor; 10 years of anatomy teaching at medical school, neuroanatomy research experience; prefer academic teaching and research opportunity. Woodward Medical Bureau, Inc., Science Division, Burneck Larson, President, 900 North Michigan Avenue, Chicago. X

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Faculty of Agriculture. The University of Alberta invites applications for a position in the Department of Animal Science as Assistant Professor, starting salary \$6000-\$7000 depending on experience and qualifications, excellent prospects for advancement. Candidates should have a Ph.D. or equivalent in animal physiology with a minor in biochemistry and be qualified to conduct research involving the use of isotopes. Duties, to commence 1 September 1960, include fundamental and applied research with farm and laboratory animals, teaching and limited extension work.

Applications, accompanied by a recent photograph and giving age, nationality and other personal information, transcripts of academic record, list of publications and experience, and names and addresses of three references, should be addressed to the Head, Department of Animal Science, University of Alberta, Edmonton, Alberta. Closing date: 30 June 1960. 5/6

Botanist. Experienced in microtechnique and general botanical preparation. For work with well-known biological supply house. Preparation of slides and other demonstrations for botany and general biology. Male or female, prefer Ph.D.; M.S. and experience acceptable. Good salary, benefits paid vacation, and sick leave. Opportunity for advancement. Reply to Box 95, SCIENCE. 5/6

(a) **Microbiologist**; Ph.D. or equivalent training to serve as head, cell production facility, conduct cell culture research; must be familiar single, suspended, monolayer cell culture techniques, related procedures; midwestern office prominent pharmaceutical company. (b) **Pharmacologist**; Ph.D. to establish evaluation program associated toxicity studies in new drug screening; report to director, pharmacologic research; to \$8500; midwestern subsidiary, prominent company; Chicago suburbs. (c) **Biochemist**; Ph.D. to train in-plant visitors in coagulation, related problems; keep abreast of competitive products, developments; give seminars in coagulation-related areas; about \$10,000. East. (d) **Bacteriologist**; B.S., M.S., for consulting firm doing drug evaluations, vitamin, hormone, toxicological assays; superior facilities; to \$6000; Midwest. (e) **Biochemist**; M.S., Ph.D., to head department, 300-bed Chicago hospital; minimum \$8000. (Please write for an analysis form. Our 64th year: Founders of the counselling service to the medical profession. Serving medicine with distinction over half a century. Science Division, Woodward Medical Bureau, Ann Woodward, Director, 185 North Wabash, Chicago. X

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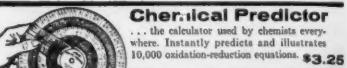
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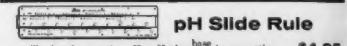
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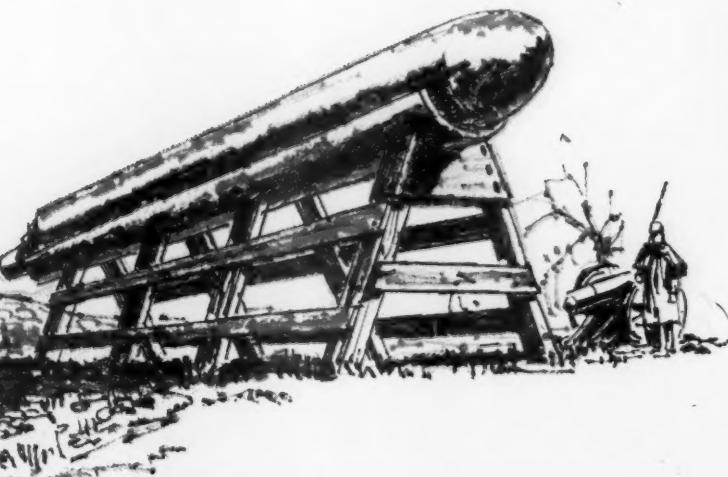
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